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THE HISTORY AND PRINCIPLES OF GEOLOGY, AND ITS AIM.

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From the earliest times the structure of the earth has been an object of interest to man, not merely on account of the useful materials he obtained from its rocky formation, but also for the curiosity awakened by strange objects it presented to his notice. The south and west of Asia, and much of the country bordering the Mediterranean, were particularly favorable for directing attention to geological phenomena. Earthquakes were of frequent occurrence, changing the relative positions of sea and land. Volcanoes were seen in eruption, adding layers of molten rock to those of sand and mud filled with the shells of the ocean. The strata in the hills abounded in evidences of similar collections of vegetable and marine life far removed from access of the sea.

The structure of the earth, however, received but little attention previous to the 7th century, B. C. The extent of the surface known was limited, and the changes upon it were not so rapid as to excite special attention. The ancient Hebrews, in the time of Solomon (1015 B. C.), prosecuted their voyages

through the Straits of Babelmandeb into the Indian Ocean, bringing home the produce of the tropical regions; while the ships sent westward to the Atlantic returned with tin, silver, lead and other metalic products of Spain and Great Britian.

The earliest idea formed of the earth seems to have been that it was a flat circular disk, surrounded on all sides by water, and covered with the heavens as with a canopy, even philosophers looked upon the earth as a disk swimming upon the water. Homer (800 B. C.) regarded the earth as a flat circle surrounded by mysterious waters. The nations that were upon its border were called Cimmerians, and were supposed to live in perpetual darkness.

As the ancients slowly gained a knowledge of the country surrounding their provinces through commercial intercourse, wars, and the search for knowledge, they were undoubtedly struck with the differences of the topography and formations. Thus geology is undoubtedly the outgrowth of geographical knowledge.

The 7th and 6th centuries B. C. were remarkable for great advance in the knowledge of the form and extent of the earth.

Their first discoveries were probably made by the Phœnicians. Their investigations were along the shores of the Mediterranean, and passing through the Straits of Gibraltar, they extended their researches into Spain and Africa and the Canaries.

Pythagoras (583 B. C.) observed the phenomena that were then attending the surface of the earth, and proposed theories for explaining the changes that had taken place in geological time. He held that in addition to volcanic action, the changes in the level of the sea and land were due to the retiring of the sea.

Aristotle (384 B. C.) recognized the interchange constantly taking place between land and sea by the action of running water and of earthquakes, and remarked " how little man can perceive in the short space of his life of operations extending through eternity of time."

Geographical knowledge was greatly advanced by the conquest of Alexander the Great (356 B. C.), in making known

Persia, and science was advanced by sending out expeditions to explore and survey the various provinces he had conquered. The Greeks he sent out, and also those who accompanied him, were critical observers and carefully described the products and aspects of the country, and made collections of all that was interesting in regard to the organic and inorganic products.

Ptolemy (323 B. C.) discovered Abyssinia and navigated the Arabian Sea, and Silineus (306 B. C.) ascended the Ganges to Batna and extended his expedition to the Indus.

It was the military genius of the Romans which led to the survey of nearly all Europe, and large tracts of Asia and Africa. In the height of their power they had surveyed and explored all the coast of the Mediterranean, Italy, the Balkan peninsula, Spain, Gaul, West Germany and Britain, and their practical genius led them to the study of the natural resources of every province and state brought under their sway.

Eratosthenes (276 B. C.) considered the world to be a sphere revolving with its surrounding atmosphere on one and the same axis and having one center. His theories were perfected by Hipparchas (160 B. C.). He attempted to catalogue the stars and to fix their relative position, and he applied to the determining of every point on the surface the same rule he introduced in the arrangement of the constellation.

Strabo (60 B. C.) noticed the rise and fall of the tide, and maintained that the *land* changed its level and not the *sea*, and that such changes happened more easily to the land beneath the sea on account of its humidity.

Ptolemy (150 A. D.) was the first scientific geographer. He followed the principles of Hipparchas, which had been neglected during the two centuries and a half since his time, even by Strabo and Pliny. In Ptolemy's work is found for the first time the mathematical principle of the construction of maps, as well as several projections of the earth's surface.

After the great achievements of Ptolemy to the 13th century, the cultivation of the physical sciences was neglected. In the 10th century Avicenna, Almar, and other Arabian writers commented on the works of the Romans, but added little of their own.

From the 13th to the 16th century, astronomy, travels, and commercial interests occupied the attention of the different nations, but geology did not appear as a separate science until in Italy in the 16th century. It began by being a record of observed facts. This was not enough, however, for it did not satisfy the demand as to how the phenomena were produced. High above sea level, and far inland, imbedded in solid rock, were found fossils. At the outset it was unfortunately linked to the belief that they were relics of the Noachean deluge. Some held that they were the result of the formation of a fatty matter, or of terrestrial exhalations or of the influence of the heavenly bodies, or that they were merely concreations, or sports of nature. The abundance of fossils in the strata of the Apennine range could not fail to arrest attention and excite inquiries. Leonardo da Vinci (1519) and Fracostaro, whose attention was engaged by the multitude of curious petrifactions which were brought to light in 1517 on the mountains of Verona in quarrying rock for repairing the city, had sound views, and showed the inadequacy of the terrestrial deluge to collect marine fossils.

Collections were made for museums, that of Canceolarius, at Verona being the most famous. Descriptive catalogues of these collections were published.

Only a few held that they were the remains of animals. Palissy in 1580, was the first who dared to assert in Paris that fossil remains once belonged to marine animals. The question was naturally asked "How came they there?" The result of investigation showed that the rocks must have accumulated around them, and hence could not always have been as they were found and that the arrangement must have changed since they were formed. This brought about the study of the construction of the earth.

Their chief objects were the examination of the materials out of which the solid framework of the earth was built, and the determination of their chemical composition, physical properties, manner of occurrence, and their characteristics. Thus they started out with the idea that rocks were made through secondary causes.

Steno (1669) observed a succession in the strata, and proposed the theory that there were rocks older than the fossiliferous strata in which organic remains occur. He also distinguished between marine and fluvialite formations. He also published his work "De solido intra solidum naturalites contento," in which he proves the identity of the fossil teeth found in Tuscany with those of living sharks.

Scilla, in 1670, published a treatise on the fossils of Calabria, and maintained the organic nature of fossil shells. But both Steno and Scilla referred their occurrence to the Noachean deluge.

In England the diluvialists were busy forming idle theories to give plausibility to their creed, that the Noachean deluge was the cause of all the past changes on the earth's surface. Differing somewhat in detail, they all agreed in the notion of an interior abyss whence the waters rushed, breaking up and bursting through the crust of the earth, to cover the surface, and whither, after the deluge, they returned. Such absurd notions greatly hindered the advance of science.

Leibnitz (1680) proposed the bold theory that the earth was originally in a molten state from heat, and that the primary rocks were formed by the cooling of the surface, which also produced the primeval ocean by condensing the surrounding vapors. The sedimentary strata, he held, resulted from the subsiding of the waters that had been put in motion from the collapse of the crust on the cooling and contracting nucleus.

Burnet (1680) published his "Sacred Theory of the Earth," and it received great applause. It was written in ignorance of the facts of the earth's structure, and was an ingenious speculation. It abounds in sublime and poetical conceptions in language of extraordinary eloquence. In 1692 he published a work which treated of the Mosaic Fall as an allegory.

Lister sent to the Royal Society, in 1683, a proposal for maps of salts and minerals. He was the first to recognize the arrangement of the earth's materials in strata, continuous over large areas, and resembling each other in different countries.

Hooke (1688) and Ray (1690), differing as much from Burnet as from Leibnitz, considered the essential condition of the

globe to be one of change, and that the forces now in action would, if allowed sufficient time, produce changes as great as those of geological time. Hooke published a "Discourse on Earthquakes," which contains the most philosophical view of the time respecting the notions of fossils and the effect of earthquakes in raising up the bed of the sea. Woodward perceived that the lines of outcrops of the strata were parallel with the ranges of mountains. He formed, about the year 1695, a collection of specimens which he systematically arranged and gave to the University of Cambridge.

They were followed in the same direction by Vallismeme (1720), Moro (1740,) Buffon (1749), Lehman (1756), and Fuchsel (1773), each contributing something additional, and advanced the most philosophical views yet presented respecting the fossiliferous strata. The first two made observations throughout Italy and the Alps. Moro endeavored to make the production of strata correspond in time with the account of the creation of the world in six days.

Buffon published his "Natural History," in which he advanced views respecting the formation and modification of mountains and valleys by the action of water.

Geology did not begin to assume the rank of an important science until its application to the practical purposes of mining and agriculture was first pointed out in 1780 by Werner, Prof. of Mineralogy in the School of Mines at Freiberg in Saxony. He greatly advanced the science by establishing the superposition of certain groups, by giving a system and names. He had very crude ideas regarding the origin of the strata. He supposed that the various formations were precipitated over the earth in succession from a chaotic fluid ; even the igneous rocks he held to be chemical precipitations from the waters.

Thus we see that the history of geology has been a record of failures, and it was not until Hutton (1788), rejecting all theories as to the beginning of the world returned to the opinions of Pythagoras and Ray. He pointed out that geologists must study the *present* if they would learn of the *past*; and he labored to show that the forces now in operation are capable

of forming rocks and of bringing about the changes that have occurred on the earth. He held that the strata which now compose the continents were once beneath the sea, and were formed out of the waste of preëxisting continents by the action of the same forces which are now destroying even the hardest rocks. Hutton was the kind of man the science had so long been in need of, and by his teaching geologists were at last started on the only path that could possibly lead them to truth. He drove out at once and forever the imaginary agencies which the early geologists had been so ready to have recourse to, and laid down the principle that in geological speculation "no powers are to be employed that are not natural to the globe, no actions to be admitted of except those of which we know the principle, and no extraordinary events to be alleged in order to explain a common appearance." He occupied himself mainly studying the changes that are now taking place on the earth's surface, and the means by which they were brought about, and in demonstrating the fact that the changes that had happened during the past periods of the earth's history were of the same kind and due to the same causes as those now going on.

The determination of the order of the strata, and the grouping of them in chronological order, were begun by Lehman (1756) and carried on by Fuchsel (1773), Pallas (1785) and Werner (1789). Smith made the most important contribution to this subject when, in 1790, he published his *Tabular View of the British Strata.* He showed their superposition and characterized the different groups by their peculiar fossils.

(*To be continued.*)

THE CONSTANCY OF BACTERIAL SPECIES IN NORMAL FORE MILK.[1]

By H. L. Bolley.

It is recognized that aside from actual dirt, as, for example, drippings from the hands of the milker, dirt from his clothing, and hairs and manurial particles from the sides of the animal, that the fore milk constitutes the most productive source of the bacterial flora of milk. Schultz and others have placed quantitative determinations at from fifty to one hundred thousand per cubic centimeter. As the character of the germ content is becoming such a matter of importance in economic labors with milk and its product, it is apparent that a consideration of the types of germ present in the normal udder should command early attention of the bacteriologically inclined dairymen.

The question is of necessity, one of such breadth that it must be approached in separate phases, such, as for example, the study of the presence or absence of physiological groups, constancy of definite species, etc. During the year just closed two such points have been under investigation. The primary object, while being a matter of simple interest, had also the direct aim of determining the relation of normal fore milk to curd inflation in cheese manufactory. The results of the work have in part been reported in a paper read before the General Section of the American Association of Agricultural Colleges and Experimental Stations, July 19, 1895; showing that, in so far as the investigation had been carried, gas generating species such as are accountable for "pinhole formation" or curd inflation are not normal to the fore milk of the healthy udder.[2]

[1] Read before the Section of Botany of the American Association for the Advancement of Science, Springfield Meeting, August 31, 1895. Also published in Centralblatt für Bacteriologie und Parasitenkunde, Ab. II, B and I, No. 22-23.

[2] Bolley and Hall: Cheese curd inflation: Its relation to the bacterial flora of fore milk. Centralb. f. Bact. u. Parasiteuk., II, Ab. I, Bd., No. 22-23.

This conclusion was based upon preliminary cheese curd tests made at Madison, Wisconsin, August, 1894, and duplicated at Fargo in October, and finally upon qualitative analysis made during a period of three winter months, with ten different milch cows under consideration.

The point to be reported upon, at this time, is that of the constancy of species as found : (a) for the same cow for a given length of time ; (b) in the same teat of the same cow ; and (c) as to whether species are common to different cows or not upon same dates.

In general, the evidence of the work associated with the last named report, was to the effect that there is no evidence that germs are of any certainty common to different animals upon the same date under like conditions ; but that a certain inhabitant of the udder of the same animal may remain quite constant. Thus while only one species, number 30, was observed to be present in more than two animals of the original ten animal test upon different dates, several different species were found to occur at several dates in the same udder.

Commencing July 1st, three animals were placed under cultural investigation, number 24 of which was an animal of the original ten, also number 21. Cultures were attempted from each teat upon gelatine and agar, as often as the work could be handled, the same methods of procuring milk being used as in the previous work, except in the different tests of the same animal, the milk tube or trochar, was inserted different depths. Some sixty of these distinct milkings were taken upon fifteen different dates, during which time the cows ran upon a clean pasture during the day, being housed at night. The milk samples were taken sometimes in the morning and sometimes at night. In all, thirty-seven different species of bacteria were separated ; and, as in past work, were found to be of various physiological types, gelatine liquifiers, non-liquifiers, solid curd types, peptonizing forms, acid and alkali curdlers, etc., including bacilli, micrococci of various forms, and a streptococcus. Thus it may be said that, in general, forms collected are miscellaneous.

Results: Again, there is no marked evidence that species are common among different animals, but there is strong evidence of constancy of appearance of certain types when once present. This, perhaps, is to be expected, for it is hardly possible that in an ordinary milking all individuals could be excluded from the milk cistern and lower teat passages.

The following table and annotations may help to show the bearings of the work:

Cow No. 24	Species present, per teat, by dates.			
Teats =	No. 1	No. 2	No. 3	No. 4
*Expr. No. 1, July 2nd.	Nos. 1	Nos. 1	Nos. 1	Nos. 5
Expr. No. 2, July 3rd.	6	1	9 and 10	5, 100 & 77
Expr. No. 3, July 4th.	16	1	15	(Not taken)
Expr. No. 4, July 6th.	(Not taken)	17 and 1	20	20
Expr. No. 5, July 8th.	(Lost Cul.)	23	10, 61	26, 27, 15, 29
Expr. No. 6, July 10th.	30, 1	(Lost)	31	(Not taken)
Expr. No. 10, July 17th.	58, 53, 1	1	61	66, 20, 15, 1
Expr. No. 13, July 23rd.	96, 93, 94	1	96, 97	20, 11, 100, 1
Expr. No. 15, July 28th.	77, 67	(Not taken)	66, 100 & 67	67, 1

*The numbers in each columns—1, 2, 3 and 4 = the laboratory numbers given the different species.

Annotation No. 1, a solid curd, lactic acid forming micrococcus, is seen to be present upon every date, appearing in teat No. 2 upon all possible dates save one.

Nos. 5, 10, 15, 61 and 67 occurred twice each, the intervening days being respectively 2, 8, 7, 4 and 4. It is worthy of note that with the exception of No. 67, each of these was found each time in the same teat.

Cow No. 21	Species present, per teat, by dates.			
Teats =	No. 1	No. 2	No. 3	No. 4
Expr. No. 8, July 12th.	Nos. 45	Nos. 31	Nos. 27, 31	Nos. 20
Expr. No. 9, July, 15th.	(Lost)	31, 50	29, 53	55,56,57,& 31
Expr. No. 12, July 16th.	53, 51, & 56	31, 45	(Not taken)	(Lost)

Annotations:—With this animal it is to be noted that No. 31, a lactic acid forming micrococcus, is constant to all dates, and upon each date was found present in teat No. 2.

Other germs found twice each were Nos. 45, 53 and 56; but each time in a different teat.

Cow No. 26	Species present, per teat, by dates.			
Teats =	No. 1	No. 2	No. 3	No. 4
Expr. No. 7, July 1st.	33, 1	33	39, 61, 67	17, 44, 33
Expr. No. 17, July 17th.	66, 100, 67 17 and 33	33, 15	33	53, 77
Expr. No. 14, July 23rd.	33	67, 33	33	(Lost)

In these three milkings from cow No. 26, the common species to each date are seen to be Nos. 33 and 67. Out of eleven milk samples taken No. 33 occurs in the cultures nine times. The intervening dates being 16, 6 and 22 days apart. No. 33 is a streptococcus and in these distant tests, as to time separation, is a strong argument of constancy of presence being possible to an individual species. In growth characteristics this germ is almost a strict anaerobe.

Studying these tables, we find for each animal the following numbered germs present:

Cow No. 24.—Nos. 1, 5, 6, 9, 10, 100, 77, 15, 16, 17, 20, 23, 61, 26, 27, 29, 30, 31, 58, 53, 66, 96, 93, 94, 97, 11 and 67, a total of twenty-seven distinct forms.

Cow No. 21.—Nos. 45, 31, 27, 20, 50, 29, 53, 55, 56, 57 and 51, a total of eleven.

Cow No. 26.—Nos. 33, 1, 39, 61, 67, 17, 44, 66, 100, 15, 53 and 77, a total of twelve.

The forms common to three animals equal only one, No. 53, while those common to two of them are seen to be Nos. 1, 100, 77, 15, 61, 29, 31, 66, 67, 29 and 20; eleven constant forms.

General Annotations:—From these summaries it is to be noted that cow No. 24 from nine different milkings furnished twenty-seven of the thirty seven germs of the three tests, *cow*

No. 21 six and cow No. 26 four. The numbered germs from the last named animals are representative of but three milking dates each. It is thus a possibility, that further milking dates for these cows might have given others of those common to cow No. 24. While this point last named, is probably a correct consideration, it is nevertheless quite clearly indicated that the great majority of germs are but incidental in a given udder or teat to the date, perhaps, to the environments of the animal. There are, however, certain few germs found which when once present in a teat or udder, remain with marked presistence. For this capability, these are found to possess what are presumably the proper physiological functions or requirements, as for example, capability to properly thrive in or withstand the normal temperature of the animal's body, and anaerobic or semi-anaerobic faculties.

As in the case of the paper previously mentioned, this is given not as final evidence to convince upon the points mentioned or suggested, but rather as a record of preliminary work accomplished.

Again, an interesting fact is the comparatively low number of species per milk sample. In the first work, winter collections, the range was from one to four species, in this it is one to five with a rather high average number. It is also interesting, though perhaps to be expected, that quantitative determinations vary from low to high numbers for different milkings, very much in accord to these last named figures.

North Dakota Experiment Station, Fargo, N. D., August, 20, 1895.

LIFE BEFORE FOSSILS.

By Charles Morris.

The beginning of life upon the earth is one of those mysteries which, to judge from what we now know about it, seems likely never to be solved by ascertained facts. There are mod-

ern facts, indeed, which bear upon it, but few geological ones, and none of absolute force. If we leave out of the question the highly problematical "Eozoon Canadense," we find the first known fossils at a comparatively high level in the rocks; and these, instead of being, as the theory of evolution requires, of very simple organization, are of a degree of development which indicates a very long period of preceding life existence. This primeval fauna, indeed, contains representatives of every branch of animal life except the vertebrate, and these not in their simplest stage, but already divided into their principal orders: the Cœlenterate class, for instance, yielding examples of Actinozoa and Hydrozoa; the Crustacean, of Trilobites and Phyllopods; and the Molluscan, of Gasteropods, Lamellibranchs and Pteropods.

This is the beginning of life as we know it. It is very far from the beginning of life as evolution demands, or as the character of the rock strata indicates. Below the Lower Cambrian beds, which contain these fossils, lie several miles of stratified rocks similar in physical character to those above them, and indicating, as Darwin says, "that during a preceding era as long as, or probably far longer than, the whole interval from the Cambrian age to the present day . . . . the world swarmed with living creatures."

Evidently we are not yet at the origin of life. We are miles away from it probably—miles of rock strata, that is. Between the simplest known microscopic creatures and the much developed Cambrian fossils an immense gap extends. The gap, for example, between a diatom and an oyster is one that represents ages of evolution; yet it is much less in extent than the yawning gap which we find dividing the line of primeval life, and which geologists have sought in vain to fill. Believers in evolution—who represent about all living scientists and the bulk of living thinkers—cannot but stand in some dismay before this strange circumstance, which must be proved away or explained away before their theory can be fully substantiated. Yet proof is not forthcoming, and only attempts at explanation remain.

In April, 1885, I presented certain views on this subject before the Academy of Natural Sciences of Philadelphia, and reinforced my arguments by later communications in 1885 and 1886. In 1894 Professor W. K. Brooks, evidently unaware of the existence of the papers mentioned, advanced a similar hypothesis in the July–August number of the "Journal of Geology," presenting a number of interesting facts, though missing, as it seems to me, much the strongest argument in defence of the hypothesis.

I propose here to repeat my former hypothesis, with additional arguments and illustrations—for some of the latter of which I acknowledge indebtedness to Professor Brooks's able paper.

To begin with, the facts of embryology may be said to point directly to what was probably the primary condition of life. The embryos of ocean animals, as a rule, begin life as swimming forms. Even the oyster—a type of sluggishness in animals—enjoys a brief existence as a swimmer before it acquires a shell and becomes permanently fixed. The same is the case with the sponge, the coral, and other stationary types, and with the various creeping or slow moving forms, such as the echinoderms. Since it has become a settled dogma of science that each stage of development passed through by the embryo represents some mature stage in the ancient ancestry of the animal, the fact stated points almost irresistably to the conclusion that the far off ancestors of the present stationary or crawling animals were swimmers—and, for that matter, naked swimmers, they being as yet destitute of hard skeletal parts.

Yet no swimming stage of existence is indicated by the oldest known fossils, or at least only by the minute pteropods and phyllopods, which were, perhaps, secondary derivatives from crawling ancestors. The trilobite may have had some swimming powers, yet probably made its way only by crawling, and the other known forms were crawlers or burrowers, or were immovably fixed. There are traces of jelly fish, it is true, but these, as they now exist, we know to be derivatives from stationary forms, and the primeval swimmers indicated by embryology have left no trace of their existence in the rocks.

Yet the oceanic waters to-day swarm with swimming life, and in all probability did so then. This life, as now existing, contains many high as well as numerous low forms. Then it must have consisted of low forms only. The wealth of existing minor sea life, as observed by the unassisted eye and revealed by the microscope, is simply boundless. Small jelly fish are met with in vast armies, hundreds of miles in extent, and descending to many feet in depth. Pteropods, both the naked and the shelled forms, occur in prodigious multitudes. The minute copepod crustaceans are found in countless swarms, and, though consumed in myriads daily by herring and other fish, by medusæ, siphonophora and other invertebrates, and even by the whale, they are so productive that their numbers seem undiminished, being found over vast areas of surface and extending through more than a mile in vertical depth. Below these again are hosts of microscopic larvæ and minute animals, and still lower are countless swarms of protozoa, such as radiolarians, globigerinæ, etc.

Here, then, are innumerable swarms of swimming and floating forms, in most part carnivorous, but necessarily requiring a vegetable basis of nutriment. The foundation food supply for such a mighty host must be enormous in quantity. The visible plant life of the ocean, the algæ which grow on the bottom, would not sustain a tithe of such an army. The microscope must again be brought into requisition, and this useful instrument reveals to us an extraordinary profusion of unicellular plants—diatoms, coccospheres, trichodesmiums, and a few other types—which extend from the surface to the lowest level of light penetration, and are so extraordinarily numerous and prolific as to supply food for all the oceanic host. These, and the protozoa which feed upon them, form the basic food supply for the countless myriads of living forms which compose the fauna of modern seas.

Yet, were the conditions of the ocean as they exist to-day to be sought for by some far future geologic delver into the mysteries of the rocks, almost nothing of this profusion of life would be revealed, discovery being nearly or entirely confined to such forms as possess hard skeletons, internal or external, of

which most of these forms are destitute. The same was probably the case with the period which we now have under review, and of whose life we find few forms except those which habitually dwelt upon the bottom. The ocean may have been as full of life then as it is to-day, many of the swimmers of that period, perhaps, representing the ancestral lines out of which the bottom dwellers had evolved, and which are still in a measure preserved for us in modern embryos. These primeval forms may have been even less suitable for fossilization than their counterparts of to-day. The diatoms, the radiolarians, and other minute existing forms have silicious shells capable of preservation. It is quite possible that the early protozoa and protophytes had no such skeletal parts, and that when they died all trace of them departed.

How far back, then, from the earliest age of fossils must we place the actual date of the origin of life? Ages perhaps—epochs—a period as remote from the Cambrian in one direction as we are in the opposite. It may have taken as long, or longer, to develop the trilobite as it since has taken to develop man. During the whole of the immensely long period in which the miles of earlier strata were being deposited, the ocean may have been the seat of an abundant life of the lowest type, and this a very slowly evolving one, the conditions being such that competition and the struggle for existence were not strongly active.

Of the forms of life now existing, the most abundant and the lowest in organization known to us are the bacteria or microbes—omnivorous life specks, feeding alike on animals and plants, and fairly assignable to neither. Possibly life had its origin in forms like these, or in still lower stages of protoplasmic activity, and from this condition developed, after an interminable period, into the simple oceanic protozoa and protophytes typified by the radiolorians and the diatoms, the lowest forms having characters common to both animals and plants, while their descendants divided definitely into plants and animals.

The period here referred to, and that subsequently consumed in the development of the trilobite and its companion forms,

must have been of very great duration; for the conditions were such as to make evolution a slow process. The habitat of these primeval life forms, the oceanic waters, was of the greatest uniformity, even probably in temperature, and possessed no condition likely to provoke rapid variation. There was abundant space and probably abundant food, particularly in view of the minuteness and slight nutritive demands of these early animals, and the struggle for existence could not have been active. Though there were millions devoured hourly, there were trillions provided for the feast, so that no great tendency towards the preservation of favorable variations would have existed.

Yet, though the influences which favor evolution were not very actively present, they could not have been quite absent. The innate tendency to vary which all living forms possess now must have existed then, and the advantage possessed by the more highly over the more lowly organized forms could not have been quite wanting. Consequently, development of varying life forms must have gone on at some rate, and animals must in time have appeared much higher in organization than the simple forms from which they emerged.

And the variations which took place were radical in character. Variation in the higher recent types of life does not penetrate deeply. After ages of change a vertebrate is a vertebrate still. Millions of years of change do not convert a cat into something radically distinct from a cat. But in the primitive period the changes were more profound. Variation went down to the foundation plan of those simple forms and converted them at once into something else. A degree of variation which now would modify the form of a fish's fin may then have converted a monad into a new type of animal. Thus primitive evolution, working on forms destitute of any definite organization, may readily have brought into existence a number of highly different types of life. As the microbe, for instance, may through long variation have given rise to the two organic kingdoms of animals and plants, so the amœba or other low animal form may have varied into the subkingdoms of mollusca, echinodermata, cœlenterata, etc., or rather into simple swimming forms

each of which was the progenitor of one of these great branches of the tree of life.

We are here in a realm of the unknown, through which we are forced to make our way slowly and uncertainly by aid of the clues of embryology, microscopic life conditions, principles of variation and development, and the known conditions of pelagic life. We can only surmise that, as the result of a long era of evolution, the simple primary forms gave rise to a considerable variety of diverse animals, still comparatively minute in size and simple in organization, swimming by means of cilia, and typified to-day by the swimming embryos of invertebrate animals.

As yet—if our hypothesis is well founded—no life existed upon the bottom of the seas, and the swimming forms were destitute of any hard parts capable of fossilization. But why did not some of these forms very early make their way to the bottom and begin life under the new conditions of contact with solid substance? And yet why should they have sought the bottom? Their food supply lay on or near the surface, the bottom of the shallow waters may have been unsuitable through the deposition of soft sediment, and the bottom of the deeper waters very sparse in food. And, more important still, they were quite unadapted to life on the bottom, and needed a radical transformation before they could survive under such conditions. If we look at the remarkable change which the swimming embryo of a star-fish or sea-urchin, for example, goes through before any resemblance to the mature form appears, we may gain some idea of the long series of variations which the primitive ciliated swimmers must have passed through to convert them into crawling or stationary bottom-dwelling forms. Great as was the period needed to produce these type forms of life, another extended period must have been necessary to convert them into well adapted habitants of the solid floor of the seas.

(*To be Continued.*)

BIRDS OF NEW GUINEA (FLY CATCHERS AND OTHERS).

BY G. S. MEAD.

Among the many kinds of Flycatchers (*Muscicapidæ*) inhabiting the Papuan Islands, while there is dissimilarity in so large a number of species, yet there are not those striking differences amounting almost to contrasts which characterize birds of greater size. Many species have been unnoticed by travellers and other writers; many exist only in cabinets and collections, labelled and ticketed, or at most given a few lines of technical summarization in catalogues. With the rank and file of birds anything more than this is impossible. Sometimes a particularly attractive specimen of *Malurus* or *Rhipidura* or *Pratincola* calls attention to itself, or mere accident brings an individual to the notice of the explorer or student.

Thus Mr. Wallace notes pointedly "the abnormal red and black flycatcher," *Peltops blainvillii*, so named by Lesson and Garnot many years since. It is a sprightly, highly colored bird with the predominant hues strongly contrasted and still further accentuated by spots of white on the head and beneath the wings. In flight this active little flycatcher presents in turn these conspicuous markings with striking effect. The red tint is a bright crimson spread over the lower back and tail coverts. The main color is a steely-green black covering with greater or less intensity the seven inches of total length. The genus is represented by this species only.

The same notable expedition to South Eastern New Guinea that secured the two beautiful prizes *Cnemophilus macgregorii* and *Amblyornis musgravianus*, discovered also a new species of flycatcher, viz., *Rhipidura auricularis*. It is described as having the "upper surface smoky gray; head brownish black; tail the same above and below; bill dark brown; legs black." The head is marked by black and white stripes, found upon the wings as well. Upon the chin, throat and breast similar

lines of unequal width are plainly drawn. The under parts are in general buff varied with black and gray. Dots and bars of white appear on the wings and tail. Its total length is about six inches.

Rhipidura leucothorax, the Whitebreasted Fantailed Flycatcher, is much more widely distributed, being met with in different parts of New Guinea. The descriptive name here describes very imperfectly, for the breast is by no means entirely white as might be inferred; black is almost as prominent, alternating with the white which shows in spaces, though lower down it crowds the black into narrow bands or crescents. The general color of the bird above is brown, becoming dark upon the head, still darker over the bill. The wings are black, finished off with white spots. This is the appearance too of the tail feathers as well as of the under side of the wings. There are also white streaks and lines about the sides of the head and throat. Bill black above. Length 8 inches.

The family of Wood Songsters (*Pœcilodryas*) are all small birds rarely exceeding 6.5 inches in total length. The coloration is in general black and white, the former greatly predominating. *Pœcilodryas albinotata* at first sight looks in color not unlike those fine drongos, the Edolias. In this instance, however, leaving the disparity of size out of account, the gray is not nearly so uniform, a dull black and a deep black appearing on the wings, tail and throat including the side face. A patch of white meets the black on the sides of the neck. White again is seen on the abdomen and under tail coverts, becoming discolored along the flanks and sides of the body.

Pœcilodryas papuana comes from the same region of the Arfak Mountains as the foregoing species. It is considerably smaller in size measuring only 4–5 inches inches in length, but of brighter color. This is a yellow, somewhat dull an becoming light brown on the wings and tail. Head and neck are darker than the body. A crescent of orange runs from the bill over the eye.

Pœcilodryas leucops shares the same habitat. It is not unlike the preceding in coloration of the body but that of the head, nape and throat is entirely different. In this case it is

a dark gray, to gray on the neck with darker feathers over the eye. White marks the upper throat and chin and appears as a prominent spot in front of the eye. Total length nearly five inches.

From the Arfak Mountains also comes *Pœcilodryas bimaculata* and from the same general region *Pœcilodryas hypoleuca* and *P. brachyura* and *P. cinerea.* The first is conspicuously black and white, the former color preponderating very largely of course, while the white shows as bands and bars or stripes. It is most apparent on the lower parts where it may be reckoned as the ground color.

P. hypoleuca, the Whitebellied, is a rather larger bird, reaching the length of 6 inches. The general color is dusky above, relieved by white patches on the head. The same color covers the under parts set off by black on breast and throat. The last named—*P. brachyura,* the Shorttailed—is marked similarly with the tones rather deeper and clearer. Length 5.5 inches.

Monachella mulleriana or *saxicolina,* a Chatlike Flycatcher, is a lively little bird found as well in the south of New Guinea along the Fly River, as in the north among the Arfak Mountains. It is of grayish plumage above becoming nearly white on the rump and tail coverts; tail feathers and wings are dark brown. The head is also dark brown with a line of white over the eye. A spot of black lies near the bill. Below the colors are nearly those of the upper parts, that is, the body is a soft white, the wings brown. Bill and feet black. The sexes are alike in markings and size, the length being about six inches. They are both assiduous in the pursuit of insects, generally along streams on level spaces.

Monarcha or *Muscipeta melanopsis,* the Carinated Gray Flycatcher, has a ring of short black feathers about the large full eye, a discriminating characteristic, imparting with the strong prominent bill a singular appearance to this Australian bird. The entire throat and part of the face are also black, crowded upon by the soft slate color which becomes deeper over the rest of the body. The long tail above is dusky; below, as well as under the wings and on the abdomen, the color is a

bright rufous. The female is unmasked about the head and throat. Feet plumbeous. Length 7 inches.

One of the loveliest, certainly the most brilliant of Flycatchers is *Monarcha chrysomela*, the Goldenhooded. Blue-black and gold are the boldly contrasted colors of this bright little creature whose length is 6 inches. The ground color is orangeyellow; this is almost equally rich whenever it is spread. Jet black with a blue gloss covers the entire throat and upper breast, the upper back, the outer wing feathers and tail. An irregular stripe of the same bends round the shoulder. The deepest black is on the throat where the thick plumage is metallic. The crown is roughened into a kind of crest. The bill and feet are black. All besides, as has been said, is a lovely yellow, making the bird a most conspicuous object among the dark trees.

Todopsis cyanocephala of the *Muscicapidæ* is adorned with a blue crown, as its name indicates. This rich color appears besides on the neck, back and wings though of a somewhat different shade. A purpleblack runs down the lower back and covers the tail, excepting the two middle feathers which are of bluish tinge. The under parts are of a dark purple also, becoming black beneath the wings. The bill and feet are dull black. The length of the male bird is rather more than six inches, the female about an inch less. Her coloring is almost as rich, but different. A warm brown takes the place of black above, a light buff of the black below, though along the sides as far as the under tail coverts the brown reappears. Blue colors the head and stripes the neck, showing lighter on the tail where it is much mingled with white.

Malurus albiscapulatus is scarcely 4 inches in length but is not only of rich velvety plumage but of conspicuous appearance also, for its white patches on a black ground color attract attention at once. These patches occur on either side of the body both above and below, those above showing finely when the bird is in flight, those below lining the chest from the bend of the wings. Elsewhere the plumage is a deep black of a bluish cast, soft and lustrous. The home of the species is in Southeast as well as Northwest New Guinea.

Caterpillarcatchers (*Campephaga*) abound in New Guinea of varying degrees of beauty, some being bright of hue, others almost somber. A few individuals not in strict order are considered here.

Campephaga sloetii or *aurulenta,* according to d'Albertis (Vide Journal), is a rare bird in collections but is distributed all over New Guinea. He found it most numerous far up the Fly River, but obtained but one specimen in a native's garden, feeding on the small berries of a tall tree. It is a yellow bird, very vivid on certain parts, duller on the wings where there is more or less black and white as well, and golden yellow on the breast and abdomen. The head, sides of head and throat are marked with gray, black greenglossed, and a band of white. White inclining to yellow lines the under wings. Bill, feet and eyes are black. The bill is short and strong.

Where the male bird is brilliant and positive in color, the female assumes paler shades and neutral tones. She is somewhat longer, measuring nearly 8 inches in total length.

The tail feathers of the male are marked with white, especially the outer ones.

The mountain Cuckoo-shrike, *Campephaga montana* or *Edoliisoma montana* is a fine bird from the Arfak region. The contrasted colors, bluegray above, black below, are so carefully marked as to render their wearer easily distinguished from his kind. The same may be said of the female who is equally conspicuous in unusually clear colors and a perfectly black tail.

The Bluegray Campephaga, *Campephaga strenua* (Schl.), from about the same region is colored mainly as its name indicates, the customary black appearing on the throat and in a line on the head. The bill and feet are also black; some of the tail feathers likewise, but a rusty tinge marks the lower wing coverts. The bill is unuusally powerful for so small a bird.

Campephaga melas or *Edoliisoma nigrum* is found in different parts of Papua. It is a larger bird and with a coloration not at all characteristic of the class to which it bears so similar a name. The male is of a glossy black, reflecting purple along

the wing and tail coverts. The female, longer than her mate in size, is also quite distinct in plumage. A marked reddish dye takes the place of lustrous black. On the head the color is warmer than on the body. The wings are shaded, in some individuals dusky.

Edoliisoma tenuirostris or *Campephaga jardinii* (Gould), the Slenderbilled Cuckoo-shrike, is an Australian bird but found also in New Guinea near Port Moresby. It is about a foot long, of a cloudy blue color, excepting on the side face where it becomes black, and on wings and tail which contain rather more black than blue. The outer tail feathers underneath terminate in white. The bill is black and anything but slender. Feet black, eyes brown.

In 1882 the nest was found by Mr. C. C. L. Talbot in a Eucalyptus tree. It was composed of wiry grasses securely fastened together with cobwebs on the thin forked horizontal branch. The eggs laid in the small shallow depression were ovoid in shape and of a pale bluish gray ground dotted irregularly over with dark brown spots and lines.

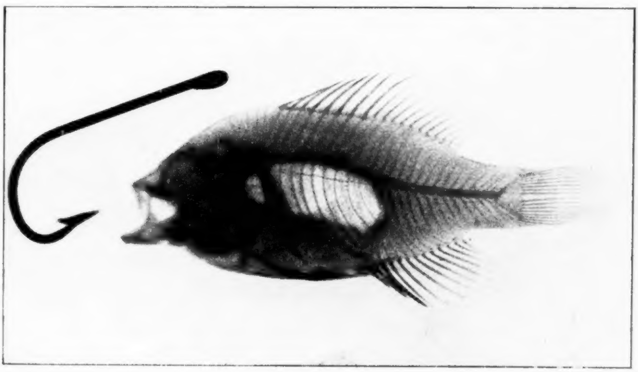
(To be Continued.)

EDITOR'S TABLE.

—WE notice that the project of a National University to be established at Washington has been again brought up before Congress. Washington has many advantages as a location for a university, and the Methodists and Catholics have not been slow to take advantage of them. The Columbian University, a non-sectarian institution, is located there. That it devolves on the nation under any circumstances to establish a university there or anywhere else we fail to perceive. So long as institutions of this kind exist either by virtue of State support or private munificence, there is no necessity for the intervention of the Government in this part of the educational field, but there are strong reasons why it should not do so. The financial basis of all institutions supported by congressional appropriations is always precarious. The subsidies are liberal while they last, but changes in the fiscal policy of the Government produce fluctuations in the revenue, and expenditures are varied accordingly. Then the faculty of such an institution would be under bonds to please the congressional majority,

PLATE V.

1



2

1. Natrix compressicauda. *2. Lepomis sp.*

or the revenues might be reduced or suspended. The teachings on certain subjects might be interfered with or controlled by the appointing power, and the appointments to positions would probably become political perquisites. Nothing more disastrous to the proper conduct of a university can be imagined, and an institution established under such conditions would soon cease to be a credit to the nation. We hope that the project will not prevail, not only for these reasons but for another. This is, that the Government has in connection with its departments various commissions and bureaus, which occupy themselves with original scientific research in connection with the various economic objects of their care. These should be continued and expanded if possible, and not, as is sometimes the case, weakened by insufficient appropriations. If the Government at Washington will support this work it will be doing more for education than any university can do, and will continue to add to its credit among nations in the future as it has done in the past.

—THE X-rays of Roentgen will prove of some utility to some branches of biological research by disclosing the characters of mineral substances enclosed within the walls of animals and plants. A good many characters of the skeleton, for instance, may be detected in specimens which cannot be spared for maceration, and other applications will occur to both botanists and zoologists. We present, as an illustration, a sciagraph of a species of sunfish (Lepomis), made by Messrs Leeds and Stokes, of Queen & Co., of Philadelphia.

—ANOTHER excellent journal, this time a French one, has been led astray by attaching too much importance to the romances of the American newspaper reporter. We refer to the story published some months ago by a San Francisco journal that a physician of that city had succeeded in grafting some snakes together by their tails. The fictitious character of the narrative is demonstrated by the statement that the said physician selected snakes in which the vertebral column does not extend to the end of the tail. If the editor of the journal had referred the question to the professors of the Museum of Paris, he would have learned that snakes of this kind exist only in the imagination of the author of the canard.

—WE published a statement some months ago that Mr. L. O. Howard of U. S. Dept. of Agriculture had discovered that the application of oil to water where mosquitoes breed, destroys both the eggs and the larvæ of those pestilent insects. We are reminded by an exchange that the alleged discovery was made by Mrs. Eugene Aaron in Phila-

delphia. We were probably indiscreet in referring to Prof. Howard's observations as involving more than a modicum of "discovery." On examination we find that the knowledge of this mode of destroying mosquitoes antedates not only his observations, but also those of Mrs. Aaron. The information has, however, not been generally disseminated until recently.

—THE American Society of Naturalists, at its last meeting, adopted a resolution commending to the public the importance of Antarctic exploration. A committee of three was appointed to take measures looking towards sending an expedition to Antarctica in the near future. At about the same time England and Australia joined in supplying the funds necessary for such an exploration of the land lying south of Tasmania within the Antarctic circle. The natural object of an American expedition is, of course, the exploration of Graham's Land, which lies due south of Patagonia. For the advance of knowledge of the physics of the globe, explorations of the polar regions are of the first importance; and the results to the history of its biology in past ages, will be scarcely less important. America has done her full share of Arctic exploration; and in the person of Commodore Wilkes made a beginning in Antarctic work. It is now fully time for us to resume this work, and it is to be hoped that the means of sending the expedition may be speedily obtained.

—THE Huxley Memorial Committee have raised the considerable sum of £1532, and are considering the uses to which it may be put. It has been resolved to erect a statue of Huxley in the British Museum, and to endow the award of a medal for meritorious work in biology. It is now desired that the amount may be increased for the purpose of creating another endowment. Should sufficient subscriptions be obtained in America, it might become appropriate that this new endowment should have its seat in this country. The scientific men of America hold in high esteem the biological work of Huxley, and there are many reasons why a foundation in his memory would be grateful to Americans.

RECENT LITERATURE.

Williams's Manual of Lithology[1] is written for the "beginner in the subject who wishes a thorough knowledge in the presentation of the subject, in a fuller and more compact arrangement than can be obtained in geological text-books. The arrangement is such that those who wish to continue the work in the microscopic analysis of rock forming minerals, as taught in petrography, will have nothing to unlearn."

The latter statement of the author is not quite true, for, in his classification of the rocks discussed, he places among the crystalline schists quartzite, pyroxene rock and olivine rock that present no traces of foliation. In the main, however, the classification is good. The rocks are divided into Primary Rocks and Secondary Rocks, and each of these groups is separated into "Divisicns" in accordance with their chemical composition. Of the different families or "divisions" the effusive rocks are first described and then the intrusive ones. The Secondary Rocks embrace the Débris, the Sedimentary and the Metamorphic divisions, the first of which differs from the second in consisting of unconsolidated materials.

Nearly all the rock varieties recognized by petrographers are briefly described, and even many that are no longer recognized as distinct types. The descriptions are all based on macroscopic characters, but they are, in most cases, full enough to enable the user of the book to identify the type.

The terminology made use of in the description is somewhat different from that used in petrographical text-books, but, since it is employed in the description of hand specimens and not of their sections, this is to be expected. All the terms used are clearly defined, and many of the new ones introduced are perhaps needed.

The main faults to be found with the volume are that it attempts to discriminate between too many rock types, and that it contains too many rock names that have long since gone out of use. In spite of these faults, the treatise is a valuable one, and it should meet with success. The typographical work is excellent. The plates are from photographs, and are illustrative of rock structures.—W. S. B.

[1] Manual of Lethology: Treating of the Principles of the Science, with Special Reference to Meguscopic Analysis. By Edward H. Williams. 2d Ed. New York: John Wiley & Sons, 1895. Pp. vi, 418; plates 6. Price, $3.00.

The Corundum Deposits of Georgia.[2]—This preliminary report on the corundum deposits of Georgia, by Francis P. King, has been issued as Bulletin No. 2 by the Geol. Survey of that State. The importance of corundum in the arts, and the high price paid for it, together with the fact that Georgia ranks second in the Union in the production of this mineral, make the report of special interest. The introductory chapters give the history, varieties and associate minerals of corundum, succeeded by a brief account of the geology of the crystalline belt in which the mineral occurs and the distribution of deposits. Several pages are given to the economics, including natural and artificial abrasives. There is also a bibliography of the American literature upon the subject.

The map accompanying the report is well-colored, showing at a glance the different formations. The other illustrations are reproductions from photographs, showing out-crops of the mineral-bearing veins.

Bailey's Plant Breeding.[3]—No man in the country, perhaps, is better prepared to write a book on plant breeding than the accomplished professor of horticulture in Cornell University, and it is a pleasure to find that in the preparation of the work before us he has not disappointed his friends. There is, as the author says in his preface, much misapprehension and imperfect knowledge as to the origination of new forms of plants, and much of what has been written on the subject is misleading. "Horticulturists commonly look upon each novelty as an isolated fact, whilst we ought to regard each one as but an expression of some law of the variation of plants." The author might have included in the foregoing many "botanists" as well as the horticulturists, for, unfortunately, it is true that many who call themselves botanists, and who hold positions in honored institutions, have not yet risen to a biological conception of the science which they profess to cultivate.

Among the topics treated in these lectures are the following, viz.: individuality, fortuitous variation, sex as a factor in the variation of plants, physical environment and variation, struggle for life, division of labor, crossing, etc.

The book should be in every botanist's library, and every teacher of botany will do well to make copious extracts from it in his lectures.

[2] A Preliminary Report on the Corundum Deposits of Georgia. By Francis P. King, Bull. No. 2, Georgia Geological Survey, Atlanta, 1894.

[3] *Plant Breeding*, being five lectures upon the Amelioration of Domestic Plants. By L. H. Bailey. New York: Macmillan & Co., 1895. pp. xii, 293, 12 mo.

The following, from page 135, will show many teachers that much may be learned from this book: "Some two or three years ago, a leading eastern seedsman conceived of a new form of bean-pod which would at once commend itself to his customers. He was so well convinced of the merits of this prospective variety, that he made a descriptive and "taking" name for it. He then wrote to a noted bean-raiser, describing the proposed variety and giving the name. 'Can you make it for me?' he asked. 'Yes, I will make you the bean,' replied the grower. The seedsman then announced in his catalogue that he would soon introduce a new bean, and, in order to hold the name, he published it along with the announcement. Two years later I visited the bean-grower. 'Did you get that bean?' I asked. 'Yes; here it is.' Sure enough, he had it, and it answered the requirements very well."

—Charles E. Bessey.

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General Notes.

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[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Mo. Geol. Survey, Vol. VIII, 1895, p. 83-222.

porphyrites and melaphyres. The author unfortunately classes as diabase-porphyrites both glassy and holocrystalline rocks. The acid rocks of the region include granites, granite-porphyries, porphyrites and quartz-porphyries. The first two are characteristically granophyric. Their orthoclases are often enlarged by granophyre material whose feldspar is fresh, while the nucleal feldspar is much altered. The quartzes likewise, are enlarged by the addition of quartz around them. There were two periods of crystallization in these rocks. In the second period the phenocrysts were corroded and the groundmass was produced. In addition to the quartz and orthoclase there are present in these rocks also biotite, hornblende, plagioclase and a number of accessory and secondary components. The porphyries and porphyrites contain the same constituents as the granites, from which they are separated simply on account of differences in structure. The phenocrysts are mainly orthoclose, plagioclase, microcline and quartz, many of which are fractured in consequence of magma motions. The groundmass in which these lie is of the usual components of porphyry groundmasses, and in texture is microgranitic, granophyric, micropegmatitic and spherulitic. Many of the porphyries contain fragments of their material surrounded by a matrix of the same composition in which flowage lines are well exhibited. These rocks are evidently volcanic breccias. The author divides the porphyritic rocks into porphyries and porphyrites, the latter containing plagioclase phenocrysts and the former phenocrysts of quartz, orthoclase and microcline.

Rocks from Eastern Africa.—The volcanic rocks of Shoa and the neighborhood of the Gulf of Aden in Eastern Africa comprise a number of varieties that have been carefully studied by Tenne.[3] The main mass of the mountains of the region consists of biotite-muscovite gneiss. This is cut by nepheline basanites, the freshest specimens of which contain phenocrysts of olivine, augite and feldspar in a groundmass of plagioclase, augite, nepheline and often olivine. Trachytes, phonolites and basalts occur in the Peninsula of Aden. The trachytes include fragments of augite-andesite. Inland granophyres with pseudospherulites in their groundmass, trachytes and feldspathic basalts were met with. The granophyres are much altered. In the fine grained product formed by the decomposition of the groundmass of one occurrence quartz, feldspar, and a blue hornblende with the properties of glaucophane can be detected. All the rocks are briefly described. They present no peculiar features other than those indicated.

[3] Zeits. d. deutsch. geol. Ges., XLV, p. 451.

A Basic Rock derived from Granite.—Associated with the ores in the hematite mines of Jefferson and St. Lawrence Counties, N. Y., is a dark eruptive rock that was called serpentine by Emmons. Smyth[4] (C. H.) has examined it microscopically and has discovered that it consists of a chlorite-like mineral, fragments of quartz and feldspar. By searching carefully he discovered less altered phases of the rock that were identified as granite. The peculiar alteration of an acid granite to a basic chlorite rock is ascribed to chemical agencies. According to the author's notion the pyrite in a neighboring highly pyritiferous gneiss was decomposed, yielding iron sulphates and sulphuric acid. These solutions passed into limestone yielding the ores and then into the granite changing it into chlorite. The altered rock is found only with the ores. The original was probably not always granite. An analysis of the altered rock gave:

SiO_2	Al_2O_3	Fe_2O_3	FeO	MgO	CaO	Na_2O	K_2O	H_2O	Total
29.70	17.03	27.15		10.66	1.68	.56	.10	11.79	=98.67

Cancrinite-Syenite from Finland.—In the southeastern portion of the Parish Kuolajaroe in Finland, Ramsay and Nyholm[5] secured specimens of a nepheline-syenite containing a large quantity of what the authors regard as original cancrinite. The rock is found associated with gneissoid granite at Pyhakurn. The rock is trachytic in structure and is composed of orthoclase, aegerine, cancrinite and nepheline as essential constituents and apatite, sphene and pyrite as accessories. The cancrinite was the last mineral to crystallize. It occupies the spaces between the other components, and yet it often possesses well defined hexagonal forms. It occurs also as little prisms included within the orthoclase. Because of this association and because the nepheline in the rock is perfectly fresh the cancrinite is regarded as original. This mineral comprises 29.04% of the entire rock.

The same authors in the same paper describe a porphyritic melilite rock found as a loose block a few kilometers W. N.-W. of Lake Wuorijarvi. It contains large porphyritic crystals of melilite, pyroxene and biotite in a groundmass composed of labradorite, zeolites and calcite. The pyroxenes are made up of a colorless augite nucleus surrounded by zones of light green aegerine-augite and deep green aegerine. No olivine was detected in any of the thin sections.

[4] Jour. Geology, Vol. 2, p. 667.

[5] Bull. Com. Geol. d. l. Finn., No. 1.

Rocks from the Sweet Grass Hills, Montana.—Weed and Pirsson[6] describe the rocks of the Sweet Grass Hills of Montana as quartz-diorite-porphyrites, quartz-syenite-porphyries and minettes. The first named rock presents no special peculiarities. The quartz-syenite-porphyry contains orthoclase, plagioclase and augite-phenocrysts in a fine groundmass of allotriomorphic feldspar and quartz. The augite is in short thick prisms composed of a pale green diopside core, which passes into a bright green aegerite mantle. The minette also contains aegerine, but otherwise it is typical.

Petrographical News.—Two peculiar phonolitic rocks are described by Pirsson[7] from near Fort Claggett, Montana. One is a leucite-sodalite-tinguaite, with leucite pseudomorphs, and sodalite as phenocrysts in a groundmass composed mainly of a felt of orthoclase and aegerine. The leucite pseudomorphs are now an aggregate of orthoclase and nepheline. In the centers of some of them are small stout prisms of an unknown brown mineral, that is pleochroic in brownish and yellowish tints. The second rock is a quartz-tinguaite porphyry somewhat similar to Brögger's grorudite.[8]

In a few notes on the surface lava flows associated with the Unkar beds of the Grand Cañon series in the Cañon of the Colorado, Ariz., Iddings[9] briefly describes compact and amygdaloidal basalts and fresh looking dolerites that are identical in all respects with modern rocks of the same character.

Laspeyres[10] estimates that the quantity of carbon-dioxide in liquid and gaseous form contained in rocks is sufficient to serve as the source for all that which escapes from the earth's natural fissures as gas, as well as that which escapes in solution with spring water. It may be set loose from the rocks through the action of heat or through the action of dynamic forces.

In a handsomely illustrated brochure Merrill[11] describes the characteristics of the onyx marbles and the processes by which they originate. Differences in temperature, according to the author, are not the controlling conditions determining the differences in texture between the onyxes and travertine. He is inclined to the belief that the banded onyxes were formed by deposition from warm solutions under pressure flowing into pools of quiet cold water.

[6] Amer. Jour. Sci., Vol. I, p. 309.
[7] Amer. Journ. Sci., 1895, Nov. p. 394.
[8] AMERICAN NATURALIST, 1895, p. 567.
[9] 14th Ann. Rep. U. S. Geol. Survey, p. 520.
[10] Korrespond. bl. Naturh. Ver. preuss. Rheinl., No. 2, 1894, p. 17.
[11] Rep. U. S. Nat. Mus., 1893, p, 539.

In a preliminary report on the Geology of Essex County, N. Y., Kemp[12] describes the occurrences of the gneisses, limestones, ophicalcites, gabbros, lamprophyres and other igneous rocks of the district, and gives an account of their geological relationships.

GEOLOGY AND PALEONTOLOGY.

Bear River Formation.—The explorations of Mr. Stanton and Mr. Charles White in the Bear River Valley have been the means of correcting a long standing error among geologists concerning the taxonomic position of strata known as the Bear River Formation. A summary of the facts as presented by Mr. White in a late Bulletin of the U. S. Geol. Survey shows that the formation under discussion is not Laramie, to which age it has been hitherto been referred, but belongs to the Upper Cretaceous, at or near the base of that series. That is its position has been determined by Mr. Stanton as beneath the Colorado formation, and above that series of Jurassic strata which occurs within a large part of the interior region of North America generally regarded as of Upper Jurassic age and which in the general section given is called "Dakota?" This accords with the reputed age of a formation in Hungary, whose fauna is more nearly like that of the Bear River series of strata than of any other known.

Mr. White, therefore, defines the Bear River series as a distinct formation stratigraphically, geographically, and paleontologically, and states in detail its taxonomic position. All the known fossils of the formation are described and figured, comparisons are made of its fauna with those of other nonmarine formations of this and other continents, and relevant biological questions are discussed.

In making a general comparison of the Bear River fauna with the other nonmarine fossil faunas of North America, Dr. White calls attention to those features of the Bear Fauna by which it differs conspicuously from all the others. Reference is here especially made to the Auriculidæ and Melaniidæ, because it is members of these two families that give the Bear River Fauna its most distinctive character. In this connection the author remarks "this faunal character is all the more conspicuous because, of the six genera which represent those two families, only two of them are known in any other North American fauna, either fossil or recent."

[12] Report of State Geologist [of New York] for 1893, p. 433.

The similarities and contrasts between the fauna of the Bear River formation and those of the other nonmarine beds of North America leads to a discussion of their causes. The author suggests that certain genetic lines of descent have become diverged from the main lines of succession and destroyed by some of those physical changes which mark successive epochs, and adds " we may reasonably assume that one of those divergent lines terminated in the Bear River fauna; that is, at the close of the Bear River epoch the area which its nonmarine waters had occupied having become overspread by the marine waters in which the Colorado formation was deposited, it is not probable that any fluvial outlet of the former nonmarine waters was perpetuated, and there was, therefore, no provisional habitat in which the Bear River fauna might have been preserved. It was probably in this way that the distinguishing types of that fauna became extinct, together with others of its members which were not so specially characteristic of it." (Bull. U. S. Geol. Surv. 128, Washington, 1895.)

On the Occurrence of Neocene Marine Diatomaceæ near New York.—The rocks which contain Diatomaceæ (or Bacillariaceæ) in America are clayey, that is to say they contain more or less of clay, and they vary in color from a nearly white to a fawn color and to a greenish, greyish-brownish or almost black. They are not older than the Oligocene nor newer than the Plistocene. They can be placed in the Neocene, a period that ranges from the Eocene to the Plistocene, and not in the recent. Those I have to describe in New York are not Miocene, but they belong to a place which may provisionally be classed as Pliocene or Plistocene of the European geologists.

Ever since 1843, the so-called infusorial earth has been known in Virginia and was thought by Rogers the discoverer to be Miocene Tertiary, he classifying it as the European rocks were. Bailey accepted the classification and so did the later geologists. When fresh water fossil Diatomaceæ were found in Massachusetts they were thought to be Miocene also without studying the rocks themselves and seeing how they stood in the geological scale. When they were found in New Hampshire I did not classify them nor did Hitchcock attempt to do so. They were placed in the lacustrine Sedimentary and provisionally in the Recent. But now they can be seen to be older than the Recent and must be placed in a position by themselves. In the Iceberg period, the Champlain, when the ice which covered the country was beginning

to melt, icebergs which formed by the breaking off of the ice on the border were common. The icy water had Bacillariaceæ in it, for they existed, as they do now, when the temperature was at 0° C. This flowed down to the lower regions from the north and northwest.

In California I did not classify the rocks containing the Bacillariaceæ leaving that to the older and more experienced geologists. Blake, who had discovered them at Monterey, supposed them to be Miocene, for he saw as Bailey showed them to be similar to the Virginian ones. In Japan where I discovered them also I failed to classify them for Pumpelly, who had brought them home did not place them likewise. When the infusorial earth was found in Florida, it had also been placed in the Miocene Tertiary by Bailey. And when I had it from that state subsequently at Manatee, I failed to classify it because I had not visited the spot where it came from myself. Now I believe these are older than what is called the Miocene. And I am confirmed in this supposition by what Towney said of the Virginia stratum. I prefer to place them as far back as the Upper Eocene, the Oligocene as it is called. In New Jersey at Asbury Park and Atlantic City the infusorial earth has been found by Woolman and classified by him as Miocene. But further north on the Atlantic side of the continent it has not been seen. I examined the clay that was dug at about two feet down at Foley's, South Beach, Staten Island, N. Y., but although it contained marine Bacillariaceæ it was not what I wanted. I thought it belonged to the Raised Coast period. At Martha's Vineyard, Mass. the clay classed as Miocene by Dall did not contain any Bacillariaceæ.

It was on the 11th of August, 1895, that I visited Rockaway to get rest from the turmoil and heat of the city. Rockaway is a beach or promontory which extends down from a place called Far Rockaway southwards on the coast of Long Island. Long Island is made up of hills of no great height extending down the middle or on the north shore of the island. A low range of country extends down the southern shore where the Atlantic Ocean begins. It is fringed by sandy bars which are mostly islands. These islands extend down the coast from Cape Cod, Mass. to Florida. Key West is the most southern of the islands which are known in Florida as Keys. The country on the Atlantic side of the island is low, sloping down to the coast without any elevation in it.

I knew that I should go down by rail cutting through the hills until I came transversely to the island to the promontory of Rockaway. It is true that I wanted to get out of the cities heat but I had also two other reasons for going. I wanted to study the glacial phenomena which I

knew would present themselves there. At the same time I desired to search for the infusorial earth. At one place we came to a kettle hole, at the Lutheran Cemetery. I was sure it was a kettle hole and knew there was clay, a Lacustrine Sedimentary deposit of Diatomaceæ, at the bottom. I saw the glacial moraine made up of gravel and sand all along the road. The moraine was a gravelly till with boulders scattered through it. On the top it was capped by a layer of about three feet thick of whitish clay. This I knew to be diatomaceous, the same as covers the country in New Jersey and on Manhattan or New York Island. As we approached the station known as Brooklyn Hills we cut through three high hills which I saw then and afterwards were made up of moraine stuff, mostly gravel, with a white clay about three feet thick on top. The clay was the same as we had just passed. It makes the bottom of the glacial clay, the Lacustrine Sedimentary deposits of Diatomaceæ. In this moraine I afterwards got a small distinctly striated boulder and near the bottom of the hill, about twelve feet from the bottom was a grey clay with Hematite nodules in it. Cretaceous clay no doubt.

The country became flat with no rising in it and sloping gradually towards the coast where we came to the station known as Aqueduct. Cretaceous clay underlies the country doubtless covered by glacial till or moraine. At Aqueduct the railroad runs out on tressels to Rockaway. At Rockaway Beach I landed and wandered south on the promontory but found nothing but white siliceous sand, they were not digging anywhere that I could find. I wandered north in the direction of Far Rockaway where the land became higher and was covered by the whitish Iceberg period clay which evidently came from the north-west. At Auvergne they had been digging a ditch to reclaim the land from the sea. This was on the opposite side of Rockaway to the Atlantic Ocean, on Jamaica Bay. The digging was over six feet deep. They had thrown out some of the Iceberg clay and below that some greyish soil without any stones in it. I saw at once that it was different in character from the soil on the marshes and which I had learned belonged to the Raised Coast or Champlain Period. I took some home and examined it and came to the conclusion that I had found what I was in search of, the infusorial earth. It was no doubt what may be termed Pliocene Tertiary and belonged to the Neocene Period.

I cleaned some and found the following Bacillariceæ in it besides some forms of *Dictyota*, which are Radiolaria. So me few usual forms escaped me but will probably be found hereafter.

Achnanthes subsessilis C. G. E.
Actinocyclus ehrenbergii J. R.
Actinoptychus undulatus C. G. E.
Auliscus cœlatus J. W. B.
Auliscus pruinosus J. W. B.
Auliscus radiatus J. W. B.
Aulacodiscus germanicus C. G. E.
Amphora ovalis F. T. K.
Amphiprora elegans W. S.
Amphiprora navicularis C. G. E.
Amphiprora pulchra J. W. B.
Biddulphia aurita A. B.
Biddulphia pulchella G.
Biddulphia rhombus W. S.
Cerataulus radiatus J. R.
Cerataulus smithii W. S.
Cerataulus turgida W. S.
Coscinodiscus asteromphalus C. G. E.
Coscinodiscus excentricus C. G. E.
Coscinodiscus subtilis C. G. E.
Coscinodiscus lineatus C. G. E.
Coscinodiscus nitidus W. G.
Cocconeis scutellum C. G. E.
Cyclotella striata F. T. K.
Dicladia mitra J. W. B.
Doryphora amphiceros F. T. K.
Epithemia turgida F. T. K.
Epithemia musculus F. T. K.
Eunotia monodon C. G. E.
Euotiogramma amphioxys C. G. E.
Fragillaria pacifica A. G.
Grammatophora marina F. T. K.
Hyalodiscus franklinii C. G. E.
Hyalodiscus stelliger J. W. B.
Isthmia enervis C. G. E.
Melosira sulcata C. G. E.
Navicula clavata A. G.
Navicula didyma C. G. E.
Navicula elliptica F. T. K.
Navicula hennedii W. S.
Navicula humerosa A. B.
Navicula lacustris W. S.
Navicula lata A. B.
Navicula peregrina F. T. K.
Navicula permagna J. W. B.
Navicula viridis C. G. E.
Nitzschia acuminata W. S.
Nitzschia balanotis A. G.
Nitzschia sigma F. T. K.
Nitzschia tryblionella H.
Plagiogramma gregoriana R. K. G.
Pleurosigma angulata W. S.
Pleurosigma balticum C. G. E.
Pyxilla? baltica A. G.
Pyxidicula compressa J. W. B.
Rhabdonema arcuatum F. T. K.
Roicosphenia currata F. T. K.
Scoliopleura tumida L. R.
Schizonema fœtida J. E. S.
Stauroneis aspera C. G. E.
Stauroneis birostris C. G. E.
Stephanopyxis appendiculata C. G. E.
Stephanopyxis turris J. R.
Surirella febigeris F. W. L.
Surirella striatula B. V.
Synedra affinis F. T. K.
Terpsinoe americana J. W. B.
Triceratium alternans J. W. B.
Triceratium favus C. G. E.
Triceratium maculatum F. T. K.
Triceratium punctatum T. B.

These are all the Bacillariaceæ that I have detected up to this time. There are several forms of *Dictyocha*, a genus of Radiolaria, present

also. And what I consider a new genus of Bacillariaceæ, which I have called *Ancile radiata.* It is free and found rarely in the salt water in Jamaica Bay, Rockaway and at Foleys, and South Beach, Staten Island. But of this I shall speak hereafter. Mr. W. A. Terry says he has found broken fragments of *Brunia* but this I myself have not seen, although common in a deposit which I will also describe hereafter taken at fifteen feet from the surface at Hoboken, N. J, I, another day, visited Coney Island, N. Y., and searched for infusorial earth and this time was fortunate enough to find it at Sheephead Bay, which is a village just on the Long Island side of Coney Island Creek. It was a grayish colored clay, one foot underneath the sand taken at low water, about eight feet from the surface of the soil. At Canarsie Landing, which is on Jamaica Bay between Coney Island and Auvergne, I did not find the infusorial earth, but I was there a very short time. I did find glacial phenomena and indication of the elevation of the coast, but of those I shall not speak now as they are not microscopical. But the finding of Bacillariaceæ in the infusorial earth, as belonging to the Upper Neocene period, is thus a fact, and the date of so finding is worthy of record. Perhaps they will be found more inland on Long Island hereafter. I have searched for them as far inland as the city of Jamaica, but without result.

This layer is in the Upper Neocene, or perhaps the Plistocene, but the placing of it definitely is extremely difficult if not impossible at present, for on describing a fossil marine Diatomaceous deposit from St. Augustine, Florida, Mr. Charles S. Boyer says (Bulletin of the Torry Botanical Club, April, 1895, Vol. 22, No. 4, page 172) that it, the St. Augustine deposit, "overlies an Eocene deposit and is beneath the Plistocene" and that the Barbadoes deposit, which corresponds partially with it, "is now claimed to be Pliocene." In fact, as I have already pointed out, the marine fossil layers of Bacillariaceæ, be it from Mors, Denmark; Simbirsk, Russia; Sentz Peter, Austria; Oran, Algiers; Moron, Spain; Argentina; Payta, Peru; New York to Virginia, California and New Zealand, including the Nicobar Islands, are Neocene, be that Miocene or Pliocene.

—Arthur M. Edwards, M. D., Newark, N. J.

The succession of Glacial changes.—Evidence has been accumulating during the last few years in favor of the periodicity of glacial action. Mr. Geikie recognized in Europe six distinct glacial epochs separated by genial periods, making in all eleven glacial and interglacial stages. For convenience he gives each of these horizons a separate name. The climax of glaciation was reached in the third

stage, that is, the second glacial epoch, after which the cold stage diminished continuously in importance. In like manner, the earliest interglacial epoch seems to have been the most genial, each successive epoch approximating more and more closely to existing conditions.

The American glacial deposits have been classified by Mr. Chamberlin, and an attempt made to correlate them with those of Europe. The following table shows the tentative correlation.

GLACIAL AND INTERGLACIAL STAGES.

European.	American.
XI. Upper Tubarian=Sixth Glacial Period.	
X. Upper Forestian=Fifth Interglacial Period.	
IX. Lower Turbarian=Fifth Glacial Epoch.	
VIII. Lower Forestian=Fourth Interglacial Epoch.	
VII. Mecklenburgian=Fourth Glacial Epoch.	Wisconsin.
VI. Neudeckian=Third Interglacial Epoch.	Toronto.
V. Polandian=Third Glacial Epoch.	Iowan.
IV. Helvetian=Second Interglacial Epoch	Aftonian.
III. Saxonian=Second Glacial Epoch.	Kansas Formation.
II. Norfolkian=First Interglacial Epoch.	
I. Scanian=First Glacial Epoch.	

The complex series subsequent to the Wisconsin formation have not been sufficiently investigated to permit even a tentative correlation, or indeed, to even designate the specific formations. This statement is equally applicable to the formations deposited during the advancing stages of the glacial period in America. (Journ. Geol., Vol. III, 1895.)

Geologic News.—PALEOZOIC.—Haworth proposes to divide the Coal Measures of Kansas into Upper and Lower, the division to be at the top of the Pleasonton shales, which is at the bottom of the Erie limestone. The division is based principally on paleontological evidence. In the author's study of the Kansas Coal Measures he finds that the shales are of submarine origin, while the entire formation appears to have been laid down during a period of gentle oscillations, with the greatest movement to the west, and the least to the east. (Kan. Univ. Quar., Vol. III, 1895.)

An Orthoceras shell of gigantic proportions has been found in the Lower Coal Measures of Iowa, about forty miles from Des Moines. This specimen is three inches in diameter and as it is of the same very slender as the associated forms, it could not have been less than six feet in length, and probably was even longer. The species is *O. fauslerensis.* (Science, Jan., 1896.)

Mesozoic.—In examining the microscopic structure of the flint nodules found in the Lower Cretaceous of Texas near Austin, Mr. J. A. Merrill found traces of the following organisms: Foraminifera, sponges, molluscs represented by the nacreous tissue of the shells, and fishes represented by their scales. The fact that the delicate spines of the sponge spicules, even to the most minute barb are perfectly preserved, showing no trace of having been subjected to mechanical movement, leads to the conclusion, that these flints result from the continuous growth of sponges *in situ*. Mr. Merrill's study then confirms to this extent the view taken by Prof. Sollas in his study of the nodules of the English flint. (Bull. Harvard, Mus. Comp. Zool., Vol. XXVIII, 1895.)

Cenozoic.—Mr. G. H. Ashley's studies of the Coast Range Mts. of California lead him to the conclusion that the east and west ranges of Santa Barbara, Ventura and Los Angeles counties were elevated at about the end of the Miocene, while the ranges to the north with a uniform strike of northwest and southeast were elevated at or near the end of the Pliocene. (Geol. Mag., Vol. III, 1895.)

Mr. A. M. Edwards reports Cenozoic clay containing marine forms of diatomaceæ from Rockaway, Long Island. The clay deposit is dark green or grey in color, and is capped by a fresh water deposit of white clay. (Observer, Dec., 1895.)

Prof. H. L. Fairchild enumerates eight reasons for regarding the Pinnacles Hills, near Rochester, N. Y. as a kame series forming a part of a frontal moraine. This is contrary to the views of Upham who considers that they were deposited "in the ice-walled channel of a stream of water," "open to the sky." (Amer. Geol., Vol. XVI, 1895.)

BOTANY.[1]

A recent paper on the relation between the Ascomycetes and Basidiomycetes.[2]—In the October number of the *Revue Mycologique* under the heading "A Fungus simultaneously an Ascomycete and Basidiomycete" appears a résumé by R. Ferry of a portion of

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

[2] Read before the Botanical Seminar of the University of Nebraska, Dec. 21, 1895.

a paper published in *Mémoires couronnés de l'Académie de Belgique,* 1894 by Ch. Bommer. I have not seen the original paper, but as Ferry gives quite a lengthy account of it and quotes the most essential parts there seems to be sufficient basis for some remarks.

The fungi under consideration are *Mylitta australis* Berk. and *Polyporus mylittæ* Cooke and Massee. The former is a large irregularly spherical hypogeous fungous growth found in Australia and Van Diemans' Land and called by the inhabitants "native bread." It was first described by Berkeley in *Ann. and Mag. of Nat. Hist.*, 1839 and referred to *Mylitta,* a doubtful genus established by Fries upon what is now known to be a gall. Berkeley says he found no spores but noticed that the ends of some of the hyphæ were swollen. No one seems to have examined the fungus for some time after Berkeley described it. According to Ferry, Tulasne regarded it as a mycelial formation analagous to *Pietra fungifera* of Battara and older writers, which is now known to be the sclerotium stage of *Polyporus tuberaster* Fr. Later Cooke and Massee[3] referring to the plant incidentally call it a sclerotium and Saccardo[4] who examined it recently, says he observed spores (?) which were globose, smooth, hyaline, plainly nucleate and 14–15μ. in diameter. Such in brief was the knowledge of the plant before the appearance of the paper under discussion.

The latter plant *Polyporus mylittæ* C. & M. (fig. 1) was first described in *Grevillea* l.c. It is a short stipitate plant with a tough pulvinate pileus about 10 cm. broad, found growing on *Mylitta australis* in southern Australia. The authors say in a note; "A most interesting production, undoubtedly the ultimate development of the sclerotium long known as *Mylitta australis* Berk."

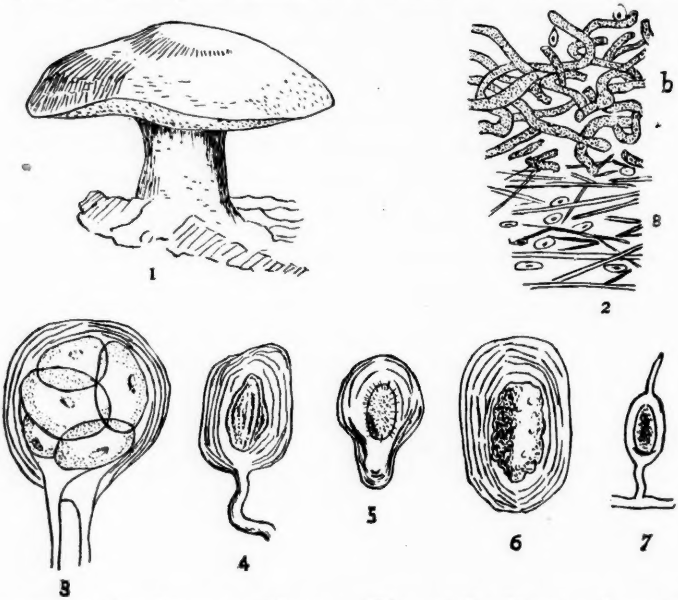
A year later Saccardo (l. c.) published a slightly different form of the same fungus under the same name. After the description he adds: "Growing on *Mylitta australis* from which it appears to originate. The texture of the *Polyporus* and of *Mylitta* are about the same. They are formed of intertwining filaments with frequent globose swellings constituting a soft or suberose white mass. It is very probable, therefore that *Mylitta* is the sclerotium form of the *Polyporus* and probably bears the same relation to the *Polyporus* that *Ceriomyces* bears to *Polyporus biennis* (Bull.) Fr."

Referring now to Bommer's paper we shall give the essential parts of Ferry's summary and translate the important parts of the quotations from the author. Ferry first gives an account of *Mylitta australis* as observed by Bommer.

[3] Cooke and Massee. Grev., **21**: 37. Dec., 1892.

[4] Saccardo. Hedw., **32**: 56. March and April, 1893.

Specimens are compact, very hard and covered with a superficial black crust. In full grown plants the interior is divided into a number of irregular cavities. The walls of these cavities are formed of a white tissue which under the microscope is seen to consist of thick-walled hyphæ which are stained by Bismark brown (fig. 2 b.). These hyphæ are from 4–8μ. in diameter. The cavities soon become filled with a gelatinous substance of a horny consistency in which some thin, hyaline, flexuose hyphæ are found buried. These are not colored by Bismark brown. Some of these hyphæ have ovoid swellings 5–8μ. long near their ends which contain 1, 2 or 3 ovoid bodies with very thin walls. Each body contains a kind of nucleus. Later these swellings (fig. 2 a.), especially those near the periphery of the gelatinous mass increase in size and contain only one ovoid body. This is brown, verrucose, very refringent, presenting all the characters of a spore and is regarded as such by Bommer. Since he finds what he considers asci and spores he refers *Mylitta australis* to the Tuberaceæ. He describes the



mature asci and spores as follows: "The asci (fig. 3) are analagous to those of *Tuber melanosporum*, being ovoid or spherical and 40–50μ. in their greatest diameter. The membrane is thin and encloses a single

elongated hyaline spore 20–30μ. long which is either smooth (fig. 4), verrucose (fig. 6) or echinulate (fig. 5)."

Nearer the centre of the gelatinous mass he says the asci are less plainly differentiated and frequently contain no spores (fig. 7). He submitted the fungus to chemical tests and found a great abundance of cellulose, but no glycogen, a substance usually present in Tubers.

The ordinary structure of the plant is, according to the author, as described above. As to its relation to *Polyporus mylittæ* which is frequently found growing from it, he says: "A specimen from the British Museum removes all doubt. This specimen like many others has a central cavity on one of the walls of which is seen a pulvinate mass formed by the hymenium of *Polyporus mylittæ*. This pulvinus does not possess true pores, but only small hemispherical cavities on its surface and numerous small rounded closed cavities in its interior which are covered by the hymenium. The mass ef hyphæ which forms the base of this hymenium is identical with the opaque white tissue which composes the walls of the cavities of nearly mature examples of *Mylitta*. Notwithstanding the presence of the pores and the thicker and more crowded hyphæ disposed after the manner of palisade tissue so characteristic of the hymenium of ordinary Hymenomycetes, the specimen is unfortunately sterile.

The particular disposition of the hymenium and the continuity and identity which exists between it and the sterile tissue of *Mylitta* establishes the fact that there exists between *Mylitta* and the *Polyporus* an intimate relation of the same nature as that which exists between the different stages in the life history of many fungi. Hence it follows that a carpophore of a Hymenomycete (*Polyporus mylittæ*) is here in reality the conidiophore of an Ascomycete (*Mylitta australis*). If this conclusion is true in the present case, it ought to be admitted that this is the relation which exists in general between the Basidiomycetes and Ascomycetes." ! ! !

Such are the author's preposterous conclusions, and thus is the autonomy of the Basidomycetes calmly disposed of.

One's first impulse is that this is a huge joke, but when you reflect that it emanates from the Belgian Academy of Sciences and is tacitly accepted by Ferry one of the editors of the *Revue Mycologique* the matter takes a serious aspect and it seems necessary to file a protest.

Ferry adds that DeBary long ago expressed the opinion that the Basidiomycetes and Uredineæ may be conidial forms of Ascomycetes. The only statement I find in DeBary[5] touching the point comes far

[5] DeBary, Comp. Morph. and Biol. of Fungi (Eng. trans.), p. 341.

from endorsing such an idea. The reviewer says further that Brefeld admits, theoretically, the existence of such fungi, but does not admit their actual existence because he does not think that two reproductive bodies of such complex development are able to be produced simultaneously. This idea Ferry regards as explaining the imperfect development of the *Polyporus* in Bommer's plant, and he adds, with a profundity equal to that of the author himself, that there are no existing characters which permit the plain separation of conidial bearing basidia (conidiophores) from typical basidia.

As to the author's observations, if accurately made they are of course deserving of consideration. We have not the space nor is it our purpose to discuss them here. Were it not for the fact that the normal form of *Polyporus mylittæ* is frequently found growing on *Mylitta*, and is regarded by several observers as being genetically connected with it, we might possibly disregard the supposed sterile hymenium and accept *Mylitta* as a tuber, though several of the author's observations are at variance with the characters of any known tuber. I am inclined for the present, however, to accept Tulasne's opinion and regard *Mylitta* as a sclerotium or a conidial stage of the *Polyporus*. As to the asci they may be illusions or may belong to some parasitic fungus. This is mere conjecture, however. These fungi are interesting forms and it is hoped that their study may be continued until the author's observations are confirmed or rejected. Accurate observations are always welcomed by botanists, but gratuitous and unfounded conclusions and generalizations should find no place in botanical literature.—C. L. Shear.

Polyporaceæ, Hydnaceæ, Helvellaceæ.—The undersigned desires species of the above groups from all parts of North America for the purpose of accumulating materials from which to monograph these families. In sending specimens, good representatives are desired, not mere fragments or abortive specimens. Where possible, indicate the host on which the fungus grows if a lignatile species, and especially in the case of fleshy or semi-fleshy forms, it is desirable to note the characters in a fresh condition. Even the most common species are desired in order to determine geographic distribution. When it is remembered that not a single species of any of these groups has been reported from more than half of our states and territories, it will be seen how great the necessity of coöperation on the part of local botanists and botanical collectors in order that this preliminary monograph may be as fairly representative of our flora as possible.

Before sending large packages, a preliminary correspondence will be desirable in order that the package can be sent the cheapest way. So

far as possible, specimens will be named for contributors and in all cases full credit will be given.—LUCIEN M. UNDERWOOD, Auburn, Ala.

The Smut of Indian-Corn (*Ustilago zeæ-mays*).—It has been found out at the Indiana Experiment Station that the smut does not attack the plant through the seed as has been supposed but like wheat rust it starts in the leaves and stems, wherever the spores are carried by the wind and find lodgment and sufficient moisture to enable them to germinate. The spores will grow as soon as ripe, that is as soon as the mass containing them turns black, and they will also retain their vitality for a year or two in case conditions for growth are not favorable.

It is evident from this that neither the time of planting nor the previous condition or treatment of the seed will have any effect upon the amount of smut in the crop. It is equally evident that meteorological conditions will have decided influence. Two things can be done to decrease smut in corn. The growing crop can be sprayed with a suitable fungicide and the entrance of the smut into the plant prevented. That this can be made effective is shown by experiments at the Indiana station. The other, more convenient but less thorough, method, is to gather and destroy the smut, and thus eventually rid the fields of it.—(*Bull. Ind. Station.*)

Antidromy and Crossfertilization.—I have been much interested in Dr. Macloskie's article on Antidromy in the November number of THE NATURALIST. It reminds me of some observations which I made several years since while investigating the subject of crossfertilization. They will be found recorded in the same journal August, 1880. A suggestion is ventured as to the possible cause of it in the flowers of *Saxifraga sarmentosa* on pages 573 and 574 of that number. In that case it seems to have little or no value in aiding crossfertilization.

Other cases, however, have been noted where it seems probable that it may be of essential value in that direction, viz., in *Solanum rostratum* and *Cassia chamaecrista.* They show lateral asymmetry, by which the pistil is on opposite sides, in successive flowers of a cluster. These plants will be found described also in THE AMERICAN NATURALIST, April, 1882.

It may be an item of general interest, also, that the features of the flowers there pointed out are so remarkable as to have attracted the attention of Darwin. He addressed a letter of congratulation and inquiry to the writer with his characteristic candor and cordiality. It may have been the last letter from that illustrious hand, for he lay cold

in death before the missive had reached its destination. He called attention also to the fact that he had observed similar asymmetry in *Mormodes ignea* and had similarly used the terms "right handed" and "left handed." The fact is published in his "Fertilization of Orchids." —J. E. TODD.

University of South Dakota, Vermillion, S. D., Dec. 2, 1895.

VEGETABLE PHYSIOLOGY.

Water Pores.—Dr. Anton Nestler contributes an interesting "Kritische Untersuchungen über die sogenannten Wasserspalten" (pp. 38, pl. 2) to Band LXIV, No. 3 of *Nova Acta d. Ksl. Leop.-Carol. Deutschen Akad. d. Naturforscher.* The term "water pore" was introduced by DeBary to designate a mechanism supposed to be distinguished from ordinary stomata by (1) Presence of liquid water, at least at times, in the substomatic opening; (2) Rigid guard cells; (3) Often very considerable differences in form and size; (4) Location near the edge of the leaf in the teeth over the end of a vascular bundle. The following subjects are considered in this paper: Previous literature; development of the water pores; structure, number, and size; rigidity of the guard cells; plants destitute of water pores. Dr. Nestler shows that water pores originate from stomatic mother cells in the same way as ordinary stomata (48 species of Ranunculus were examined and also plants of many other families); that while water pores sometimes exceed ordinary stomata in size they are quite as often of the same size or smaller, and frequently show plain transitions into the latter; that rigidity of the guard cells is not always present in the water pores nor always absent in ordinary stomata; that water pores sometimes discharge vapor of water; and, finally, that the ordinary stomata sometimes, and probably often, excrete liquid water (over the whole upper surface of the leaf in *Vicia Faba*).—ERWIN F. SMITH.

Biology of Smut Fungi.—The third part of Dr. Brefeld's Smut Fungi (Heft XII of the *Untersuchungen*) contains 140 pages of quarto text and 267 figures packed into 7 lithographic plates, the crowding together of which makes difficult the comparison of text and figures. All told 13 genera and 64 species are described, of which latter 22 are reckoned as new. The germination of the smut spores is figured for most of the species as well as described. The descriptions are long and include a wealth of biological detail drawn from the behavior of the

various forms in Nährlösung. Two new genera are established: Arthrocoidea, founded on two old species (*Ustilago subinclusa* and *U. carycis*), separated from Ustilago by peculiarities of germination, and Ustilaginoidea, a most peculiar genus, founded on Patouillard's *Tilletia oryzæ* and on a new species found by Möller on *Setaria Crus-Ardeæ* in Brazil. Material for the study of the fungus on rice was obtained from Barclay in India. This fungus which causes a swelling of the ovaries of the rice plant to several times the normal breadth of the grain and which has the external appearance of a smut, has nothing to do with Tilletia, but seems to belong to some other group of fungi. Its principal peculiarities are (1) the production of a large number of smutlike spores on the outer part of the transformed grain, the interior of the same being occupied by a hard mass of nonsporiferous hyphæ suggesting an immature sclerotium; (2) germination in a manner totally different from that of any other smut spores and resembling that of some Ascomycetes, i. e. by the development of a much branched septate mycelium which, in dilute Nährlösung, bears succedaneously on the ends of the hyphæ, small, oval, colorless, nongerminating conidia, and in concentrated Nährlösung omits these conidia and develops in their stead and also anywhere on the walls of the hyphæ, sessile dark greenish-black, echinulate, thickwalled spores one in a place or sometimes two together, one above the other. In the species received from Brazil most of the dark spores had fallen off and the development of the central mass of hyphæ had proceeded a step further, being changed into a true sclerotium with a black rind and an internal thickwalled white pseudoparenchyma. Additional facts are promised as soon as these sclerotia can be induced to germinate. The descriptions are followed by a discussion of the relationship of the smuts to each other and to other fungi. A full account of culture methods and some additional notes on fungi are promised for Heft XIII to appear soon.

Incidentally Dr. Brefeld pays his compliments to the perfunctory grinders out of species: "The accidental circumstance that the all naming Patouillard has given to the fungus on rice the name *Tilletia oryzæ* shows once more how worthless are the namings of a spore material without the developmental history. The latter shows that in Patouillard's supposed *Tilletia oryzæ* we have to do not with a Tilletia and not even with a smut fungus but with a form out of the highest group of fungi." This is quite to the point. The labors of the "all naming" mycologists of the past have filled this part of systematic botany with a mass of rubbish mountain high, and still the brave work goes on, exactly as if it were not known that fungi are exceedingly

variable organisms, or that it is possible by holding on to the old notion of fixity of species to make half a dozen new ones out of the product of a single spore by a little variation of the substratum, or even without the latter device by drawing up separate descriptions of old and young and large, small and medium sized spores. Is it not indeed time we should have a reform and begin to *reduce* the number of species by carefully studying those which have been badly described (by far the larger number), learning their life history and the extent of their variability under ordinary conditions, and throwing out the synonyms? This method carefully applied would unquestionably reduce the number of so-called species of fungi and bacteria nearly or quite one-half. This must necessarily form a large part of the work of the next generation of mycologists, and no one familiar with the ground can doubt that the task of properly classifying these plants would be immensely easier if half the descriptions had never been written.—ERWIN F. SMITH.

Function of Anthocyan.—The following is an abstract of a short paper by Prof. Leopold Kny, of Berlin, Zur physiologische Bedeutung des Anthocyans, published in *Atti del Congresso Botanico internazionale di Genova*, 1892 (pp. 135–144). The name anthocyan has been given to a coloring matter occurring in the vegetative and floral organs of many plants in numerous transitional shades from red through violet to blue. It occurs dissolved in the cell sap and is sensitive to acids and alkalies, changing from one shade of color to another as they are used. It is probable that several different substances have been included under this term, for while in most plants these colors appear only on exposure to light, especially bright sunshine, in others they appear just the same in total darkness, e. g. in the perianth of *Tulipa gesneriana*, *Crocus vernus*, and *Scilla siberica*, the inner tissues of the root of the red beet, and the inner leaves of the red cabbage. In case of the floral organs anthocyan undoubtedly serves to make them conspicuous to insects, etc., but for the most part it can have no such function in the vegetative organs. Its use to these parts of the plant has been explained in three different ways. (1) When young leaves and stems either from seedlings or from buds take on a distinct red or violet color and subsequently lose it wholly or in part, it is but a step to the hypothesis that this color has been developed for the protection of the chlorophyll from injury by light. It is explained in this way by Kerner von Marilaun. On this supposition, it is difficult to understand how many young shoots get along without it, e. g. species of Iris, the young leaves of which are bright green. As proof, Kerner makes

prominent the abundance of anthocyan in many alpine plants as well as the fact that when a species grows on the plains as well as in the mountains it is in the latter locality that the vegetative and floral organs show an inclination to become red with anthocyan. (2) In cases where the cells holding the anthocyan are on the under side of the leaf, the upper side being pure green (*Cyclamen europæum, Hydrocharis Morsus ranæ*) the lightscreen hypothesis naturally falls to the ground. Here there is every reason to believe, according to Kerner, that the light rays which would otherwise pass out of the plant and be lost are converted into heat rays in passing through the cells containing anthocyan. In conformity with this hypothesis we find that the leaves of trees and shrubs which are lifted up from the soil and have other green leaves below to catch the filtered light, are never violet on their under surface, while, in very leafy under shrubs, only the lowermost leaves next the ground are provided with anthocyan. Another indication of the warming influence of anthocyan is its abundance in alpine plants, as already mentioned, and its frequent development in the perennial leaves of other plants during the winter season (*Sempervivum tectorum, Ligustrum vulgare, Hedera helix, Mahonia aquifolium*) the leaves being enabled thereby, in sunny winter days, to break up carbon dioxide even at relatively low temperatures. (3) There are, however, a series of facts going to show that the preceding hypotheses are not sufficient to explain all cases. On full grown shoots of many herbs and woody plants the sunny side of the internodes frequently becomes red while the opposite side remains nearly or quite pure green (*Salix* species, *Polygonum fagopyrum*, and many other plants). The same difference is frequently observed on petioles, the red color being not rarely prolonged into the midrib and its branches. These facts lead to the conclusion that the screen of anthocyan may have some use in connection with the breaking up and translocation of plastic substances through the vascular system. This is also indicated by the fact that when the roots of willows and other plants grow down from a bank into the water and are subject to direct sunlight they become red on the exposed surface. Pick considers the anthocyan screen as a means of bringing about the outward movement of starch in large quantities without seriously disturbing the assimilatory activity of the chlorophyll bodies. Some effort has been made to demonstrate this third view, but so far as known, no one has tried to establish the first two by means of experiment. The following experiments were, therefore, undertaken to fill this gap. (1) *Does anthocyan protect chlorophyll from the destructive action of light?* Owing to the manifest difficulty of dealing directly with the chlorophyll bodies the experiments were made

with an alcoholic solution derived from grass leaves. Two beakers were filled with this green solution and placed in tin chambers with blackened inner walls but having on one side a quadrangular opening with strongly projecting edges for the entrance of light. In front of each opening was placed a parallel walled glass vessel 196 millimeters high, 93.5 mm. wide and 40 mm. thick. Into one of these vessels red beet juice was poured and into the other white beet juice, both filtered and of the same specific gravity. The result was decisive. The light which passed through the anthocyan solution discolored the chlorophyll much less rapidly than that which passed through the colorless solution. (2) *Does anthocyan convert the light rays into heat rays?* Experiments were made with the foliage of green and red leaved varieties of the following species, viz. *Fagus sylvatica, Corylus avellana, Berberis vulgaris, Acer platanoides, Brassica oleracea, Dracæna ferrea, Canna indica;* with decoctions of white and red beets; and with the petals of a white and a red rose. Exactly weighed quantities of the leaves, etc., were placed in the parallel walled glass vessels already mentioned, thermometers were then plunged into the center of the mass, and the vessels were exposed to the action of direct sunlight filtered through a nearly saturated alum water screen 4 cm. thick (to absorb the heat rays). In most of the species (*Dracæna ferrea* and *Canna indica* gave contradictory results) the ability of anthocyan to convert light rays into heat rays seems to have been demonstrated conclusively. In one to two minutes in favorable cases there was a rise of temperature in the vessels containing the red leaves, the maximum difference amounting to as much as 4°C. As soon as the sun was covered by a cloud there was a noticeable fall of temperature in both vessels, and when the cloudiness lasted 10 to 20 minutes the temperature became the same or nearly the same in both vessels. Subsequently an effort was made to determine whether the different light rays of the solar spectrum behaved differently. For this purpose three vessels containing, in turn, red leaves of several species of plants were exposed to direct light under the following conditions; the light entering one vessel was filtered through the alum solution, that entering another was filtered through a screen of sulfuric-copper-oxide-ammonia, that entering the third was passed through a solution of bichromate of potash, it having been determined in advance spectroscopically that the two colored screens divided the spectrum in about the middle of the green. Under these conditions the rise of temperature was less behind the blue screen than behind the orange one, and less behind the latter than behind the alum screen. A consideration of the third supposed function of anthocyan is left by Dr. Kny for a subsequent paper.—ERWIN F. SMITH.

ZOOLOGY.

A posthumous paper on Myxospridia by M. Prosper Thélohan has recently appeared prefaced with a short account of the authors's scientific career by E. G. Balbiani. The Memoir, intended as a thesis for the degree of Doctor of Science, while complete in the essential parts, lacks the final chapter in which the author intended to indicate the relations of the different genera and families of the Myxosporidies.

Briefly stated, Myxosporida are parasitic Sporozoa found living in certain fishes, batrachians and reptiles. They have also been observed living in various arthropods, notably spiders and crustaceans. Certain families are limited to vertebrates host: Myxobolidæ and Chloromyxidæ. It is to the latter forms that the author devotes his paper.

It has long been known that the Myxosporida of the vertebrates assume two forms; one, a small ameboid body swimming free in the liquid which contained in certain organs, chiefly the gall and urinary bladders, and a second form which is found distributed in compact tissues, like the connective tissues and the muscles. In either case they may be harmless to the host, or on the other hand, give rise to grave disorders, resulting in the death of the animal which they have invaded.

The free swimming species are variable in form, the most common one being that of an elongated cone the base of which corresponds to the anterior extremity; others are almost spherical. It is, however, difficult to decide upon a definite species form, since each individual exhibits such extraordinary polymorphism. The organisms found in the tissues are generally spherical.

Ordinarily these parasites are colorless, but yellow ones have been seen, and a few green ones are reported.

In dimensions, as in form, there is great diversity. The free swimming species are from 10 or 12μ. in diameter to 5 mm. in diameter.

Reproduction is accomplished by sporulation, and, probably, also by fission. The protoplasmic body of the Myxosporida is plainly differentiated into a peripheral zone, *ectoplasm*, surrounding the central sarcode, *endoplasm*. The former functions as a protection for the latter and, also is capable of putting out pseudopodia which act as organs of locomotion or fixation. These pseudopodia are localized in certain species, in others they appear at random. They take no part in the phenomena of nutrition.

The endoplasm of young individuals appears homogeneous, but in older ones there are found, in some cases, certain products of differentia-

tion, among which the author distinguished, fatty globular masses and rhombohedral crystals of hæmatoidin. In others, there are vacuoles, containing protoplasmic matter which differs from the rest of the endoplasm. It is in the endoplasm also that the nuclear elements are found, often in great number, around which the spores develop. The author traces the development of these spores, describing minutely the various stages of growth. Upon arriving at maturity they remain enclosed in the endoplasm for a varying length of time. When set free it seems to be connected with the destruction of the protoplasm which persists in the mother organism after the formation of the spores. The free-swimming species are expelled from the host either with the fæces or the urine, but the ones imprisoned in the tissues continue where they are until set free by the death and subsequent decay of the tissues of the host. The spores rarely germinate in the old host, never in any exterior medium, but stay dormant until chance provides them a new host.

As to the food habits of the Myxosporida, M. Thélohan observations are to the effect that they imbibe nourishment from the fluids in which they live. In no case did he see food particles ingested.

The following classification of the Myxosporida was proposed by the author in 1892, and his subsequent researches confirms the distinguishing characters.

Spores	form variable	no vacuoles in the plasma.	2 capsules.	I. Myxidiidæ.
			4 capsules.	II. Chloromyxidæ.
		1 vacuole which colors a reddish brown by iodine.		III. Myxobolidæ.
	pyriform, a single polar capsule, not easily seen, with a pointed extremity; a clear vacuole, not colorable with iodine, at the larger end.			IV. Glugeidæ.

Myxidiidæ contains 6 genera with 25 species; Chloromyxidæ has 1 genus, 6 species; Myxobolidæ 2 genera, 14 species; Glugeidæ 3 genera, 16 species.

The Segmentation of the Hexapod Body.—In a recent paper[1] giving the results of work upon the early stages of certain of the Orthoptera, Dr. Heymons gives the whole number of segments in the Hexapod body as twenty-one, of which six form the head; three, the

[1] Anhang. Abh. K. preuss. Akad. Wiss., Berlin, 1895.

thorax ; and twelve, the abdomen. At some time during the development of the insect, appendages are present upon all except the first, third and twenty-first segments. The frons, clypeus, labrum and compound eyes are parts of the first segment. The second segment bears the antennæ, the fourth the mandibles, and the fifth and sixth the two pairs of maxillæ. The hypopharynx does not belong in the series of appendages but is formed by a folding of the ventral portions of the fourth, fifth and sixth segments. The cerci, contrary to the views of some authors, are the true appendages of the twentieth (eleventh abdominal) segment. Considerable emphasis is laid upon the similarity between the first and twenty-first segments, in their relations to the openings of the alimentary canal, in being free from appendages, in the lateral position of their ganglia and in the relative changes of the appendages of the adjoining segment. Concerning the position of the genital openings, Heymons reiterates his former opinion that they may belong primitively to the tenth segment, their position in the ninth being a secondary development.—G. M. WINSLOW.

The Coxal Glands of Thelyphonus caudatus.—In a brief note in the Zoologischer Anzeiger,[2] Dr. Theo. Adensamer adds a few facts to complete Sturany's work on the Arachnoidea. The two glands occur between the gastric cœca and the muscles, and extend as unbranched and unlobed sacs to the abdomen. From the anterior end of each extends a simple duct to the coxæ of the first pair of legs through which they open. A thin chitinous intima was distinguished in the ducts. An histologically differentiated portion of the gland corresponding to Lankester's medullary substance and Sturany's Marksubsanz was not found.

The following table shows the location of the openings of the glands in the several groups:

Limulus, openings in the 5th appendages.
Scorpio, openings in the 3d pair of legs = 6th appendages.
Pseudoscorpionidea, openings in the ? = ?
Thelyphonus, openings in the 1st pair of legs = 3d appendages.
Araneida:
  a. Tetrapneumous, openings in the 3d pair of legs = 5th appendages.
  b. Dipneumous, openings in the 1st pair of legs = 3d appendages.
Phalangida, openings in the 3d pair of legs = 5th appendages.
Acarina, openings in the ? pair of legs = ? —F. C. K.

[2] XVIII, p. 424.

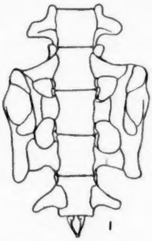
Cross Fertilization and Sexual Rights and Lefts Among Vertebrates.—The November number of this journal, page 1012, under the title "Sexual Rights and Lefts," called attention to sexual peculiarities I had recently discovered in certain Cyprinodonts. At that time no satisfactory explanation of the purpose or origin of the strange conditions offered itself. At present I would like to note in these pages what upon further consideration appears to me the best solution of the problem. Additional study has satisfied me that the sexual conditions in the genus Anableps prevent close "inbreeding," or, in other words, they secure cross fertilization. What in certain plants is attained by means of short stamens with the long ones is in these fishes realized by sinistral and dextral males and females. This is a view in the case of Anableps that brings us in face of probable benefit from the novel features, and of the possible causes of their evolution. As bearing on the inception of the dextral and the sinistral peculiarities we must consider the habit possessed by so many of these fishes of swimming in pairs, side by side, a habit that induced Professor Agassiz to name one of the genera *Zygonectes,* that is yoke swimmers. The acquisition of more or less of a dextral or of a sinistral tendency would not be at all unnatural in each of a pair habitually swimming side by side in the same relative positions to one another. It may be that cross fertilization will afford an explanation of conditions somewhat similar among molluscs.

While writing of matters concerning the publication "The Cyprinodonts," it should be mentioned, as kindly pointed out to me by Dr. A. Smith Woodward of the British Museum, that the name of one of the new genera, *Glaridodon,* was recently preoccupied among fossils, and it may be well here to discard that name (p. 40) for the term *Glaridichthys.*—S. Garman, Cambridge, Mass.

Abnormal Sacrum in an Alligator.—Among a lot of young alligators procured from New Orleans for the University of Chicago one in which the skeleton was prepared, showed a very peculiar variation in the pelvic region there being three instead of two sacral vertebræ.

There are as usual 24 presacral vertebræ. The 25th has the sacral ribs inclined backwards and becoming slender. The 26th has strong thick ribs, and the 27th, the first caudal in normal specimens, has also well developed ribs articulating strongly with the ilium. The 27th is seemingly biconvex. The first chevron is attached between the 28th and 29th and is, therefore, in the normal position as regards the serial number of the vertebræ, but is attached to the first vertebræ the last sacral instead of the second. The whole pelvis has migrated backwards one

vertebræ, the first true sacral tending to become a lumbar and the first caudal has become a sacral. The two side are strikingly symmetrical. The figures giving views from above and below are natural size and include the 24th–28th vertebræ.



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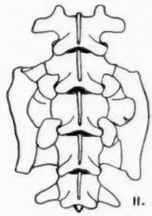


II.

The other known cases of variation in the sacrum of Crocodilia are, as far as I am aware, as follows: Rheinhardt[1] examined 11 specimens and found 3 abnormal.

1. *Alligator sclerops* Schn.: Last lumbar become a sacral; 23 presacrals.

2. *Crocodilus acutus:* 3 sacrals, 3 plane-convex, 1st caudal concave-convex and bearing a chevron, thus the first caudal has become a sacral, 23 presacrals.

3. *Crocodilus acutus*: First caudal has become a sacral, 24 presacral.

Baur[2] reported two cases.

1. *Gavialis gangeticus:* 25 presacrals. One intercalated between the 9th and 10th.

2. *Alligator mississippiensis*: Last lumbar become a sacral, showing on one side a small sacral rib and which does not reach ilium, 23 presacrals.

Baur[3] reported three cases.

1. *Crocodilus acutus:* A specimen in the museum at Cambridge, Eng. shows on the right side of the 25th vertebra a strong and separate rib, on the left side the rib is smaller and coössified with the centrum. The 26 shows typical sacral ribs. The 27th shows on the left side a

[1] (Anomalier i Krydsvirvlerne hos Krokodelerne, Copenhagen, 1873, and Sur les anomalies des vertèbres sacrées chez les crocodilieus. Jul. de Zoologie T. III, No. 4. Paris, 1874.)

[2] Zoologischer Anzeiger, IX Jahrg., No. 238, 1886. Osteolog. Not. über Reptilen.

[3] (Zoolog. Anz. XII, Jahrg., No. 306, 1889. Revision meiner Mittheilungen in Zoologisher Anzeiger, mit Nachträgen.)

strong free rib and on the right side a weaker rib but free. The 28th biconvex.

2. *Crocodilus acutus*: Two specimens in the Royal museum at Leiden have only 23 presacrals.—E. C. CASE.

The Polar Hares of Eastern North America, with Descriptions of New Forms.—In 1819 Captain John Ross, in the fourth Appendix of the second (octavo) edition of his "Voyage of Discovery" in Baffin's Bay, described a hare which he procured in Baffin Land, in latitude 73° 37′.

To this animal he gave the name "*Lepus arcticus* Leach," stating at the end of his description that "Dr. Leach thinks it to be very distinct from the common White Hare of Scotland (*Lepus albus* Brisson) and equally so from the *Lepus variabilis*, Pallas." Ross then makes a reference to "Appendix No. V," of the same volume, which he evidently supposed would contain Leach's description of the same animal. Leach's chapter on the "New Species of Animals" obtained by Ross, however, does not come in appendix number five but is part of the same appendix in which Ross' description appears. It is on page 170, while Ross' description is on page 151. Leach evidently described the same specimen which Ross had in hand, but gave it the name *Lepus glacialis*. Owing to its precedence in paging, Dr. J. A. Allen[1] rightly adopts the name *arcticus* for the American Polar Hare, *glacialis* of Leach becoming a sýnonym.

The question has been raised by my friend, Mr. Outram Bangs, whether Ross, and not Leach, should have credit for the name *arcticus*. We may justly infer from Ross' description that he intended that Leach should have this credit and that he published it with such intention. He must have consulted with Leach about its relations to the European and Scottish Hares and quotes Leach in his diagnosis, using, without doubt, the specific name then suggested by Leach. The fact that Leach gave it another name does not affect the status of the one given by Ross, nor weaken Leach's claim to it. From the present custom, not definitely formulated in our American Ornithologist's Union's canons of nomenclature, I see, however, no alternative but to call the Baffin Land Hare, *Lepus arcticus* Ross.[2]

[1] Mon. N. Amer. Rod., 1877, p. 288.

[2] Some authorities prefer that sole credit for the name of a species be given to the person to whom the original publisher of that name ascribes the origin of the name, writing it in this case *Lepus arcticus* Leach. The A. O. U., with one (or two ?) exceptions, adopts the reverse rule in their check list of birds, and would make it read *Lepus arcticus* Ross. Neither method does justice either to the public or to

Dr. J. A. Allen (l. c.) concludes that the American Polar Hare is not specifically separable from the European *L. timidus* (=*variabilis* Auct.), and the deficient material which he had for examination at that time probably justified such a verdict as the safest one, especially when we consider the standard of species and varieties adopted at that date by American mammalogists. Through the kind liberality of Messrs. G. Brown Goode and F. W. True of the Smithsonian Institution, and of Mr. Outram Bangs of Boston, I have been favored to examine, in connection with the specimens in the Academy of Natural Sciences of Philadelphia, an unusually large series of skins and skulls of the Polar Hares of America and northwestern Europe. The results of this study, so far as they relate to the Polar Hares of eastern North America, and Scandinavia may be summed thus briefly.—

1. Lepus timidus L. Scandinavian Polar Hare.

Type locality (hypothetically restricted), Southern Sweden.

Nasals nearly or quite reaching to anterior vertical plane of premaxillaries. Posterior frontal swelling on a plane with the postorbital processes. Upper incisor with transverse sectional diameter greater than the longitudinal diameter; the chord of the arc of its exposed surface (with skull, minus mandibles, resting on a plane horizontal surface) is vertical; the radius of the arc described by the incisors is one-eighth ($\frac{12}{100}$) of the basilar length of skull; their inner faces indented by a deep broad sulcus and they are rooted on the premaxillaries at or slightly anterior to the inferior maxillo-premaxillary sutures. Roots of lower incisors extending to base of $\overline{\text{pm. 1}}$.

Summer pelage; above blackish brown, sprinkled with gray; ears darker, but not black, tail white, dark above.

2. Lepus arcticus "Leach," Ross. Baffin Land Polar Hare.

Type locality, lat. 73° 37′, northern Baffin Land, southeast of Cape Bowen.

Size larger (?) than *timidus*, with relatively smaller and wider skull and shorter ears. Skull of the same type as *timidus*, with the following differences: Nasals, rostrum and incisive foramina relatively

those personally interested. I suggested (Proc. Acad. Nat. Sci., Phila, 1895, p. 395), that both the publishing and the manuscript or verbal authority for such names should be indicated. My friend, Witmer Stone, has suggested an improvement on my formula which I heartily endorse, viz.. that instead of "*Rana clamitans* Bosc., Mss., Sonn., Latr." (l. c.), it should read *Rana clamitans* "Bosc.," Sonn. & Latr., and the Baffin Land Hare would read *Lepus arcticus* "Leach," Ross. This comports far better with our motto that, "Zoological nomenclature is a means, not an end, in zoological science."

shorter and broader, the incisive foramina never reaching to middle of pm. 1. Palatal bridge longer than width of postpalatal fossa. Supra orbital processes of frontals deeply notched anteriorly, upraised and widely flaring. Frontals, at their posterior constriction, remarkably tumid, their anterior plane greatly depressed.

Summer pelage (fide Ross and Leach (l. c.) and Sabine[3]), white, "The back and top of the head are sprinkled with blackish brown hair which is banded with white, the sides of the neck are covered with hairs of the same color, interspersed with white. The extreme tips of the ears are tipped with black."—Leach. "In some of the full-grown specimens, killed in the height of summer, the hair was a grayish brown towards the points but the mass of fur beneath still remained white, the face and front of the ears were a deeper gray."—Sabine.

In south Baffin Land, as evidenced by a specimen from Cumberland Gulf, the type form intergrades into the following subspecies:

3. Lepus arcticus bangsii Rhoads, subsp. nov. Newfoundland Polar Hare. Type locality, Codry, Newfoundland. (Diagnosis as given below.)

4. Lepus grœnlandicus Rhoads, sp. nov. Greenland Polar Hare. Type locality, Robinson's Bay, Greenland. (Diagnosis as given below.)

Lepus arcticus bangsii,[4] subsp. nov. Newfoundland Polar Hare.

Type, Ad. ♀, No. 3752; Col. of E. A. & O. Bangs. Collected by Ernest Doane at Codry, Newfoundland, Aug. 3d, 1895.

Description.—Size equal to *L. timidus* L., of Southern Sweden, with shorter ears, shorter and broader skull, nasal bones and incisive foramina, weaker dentition and narrower frontal breadth anterior to the supraorbital processes.

Adult summer pelage: entire back and upper sides, including neck, shoulders and outer surfaces of thighs, uniform dark grizzled gray, faintly suffused with tawny. A pinch of hairs from near middle of back shows the following color pattern: under-fur fine, tawny-white basally, becoming tawny at distal end; over-fur white or black at base in about equal proportions, the coarser black-based hairs black throughout, the finer white-based hairs with terminal half black, interrupted by a subterminal band of white or pale tawny. Lower head

[3] Suppl. Appx., Parry's Voy., 1824, pp. 187–188.

[4] Named for Mr. Outram Bangs, who has done so much in making known the mammal fauna of Newfoundland, and who has there collected the finest study-series of Polar hares that can be found in this country.

(including chin), lower neck, nape, forebreast to forelegs, lower sides, edges of thighs and rump, dark plumbeous gray, flecked with very long, slender, white hairs. Lower breast, belly, vent and tail white, bordered by a nearly clear plumbeous edging which separates the ventral from the abdominal regions and joins the dark rump along the inside of thighs. Inner anterior border of hams, sides of hind feet and toes, and lower surfaces of forelegs, white, thinly intermixed with leaden hairs. Outer surfaces of fore and hind legs and superior surfaces of the feet, tawny gray. Ears and space between them, black, becoming grayish at base and with a narrow whitish outer posterior margin from near base to tip. Upper head, including cheeks and nose, grizzled buffy gray, appreciably lighter than the gray shades of the back. Eyelids whitish, edged with black. Whiskers weak and sparse, white and black in equal proportions, the longer black hairs tipped with white.

Winter pelage (No. 1187, Ad. ♀, Col. of E. A. & O. Bangs. Bay St. George, Newfoundland, Mar. 1, 1895): Entire pelage, exclusive of ears, white. Extreme tips of ears black, the median anterior borders of ears grayish.

Measurements (of type).—Total length, 626 millimeters; tail vertebræ, 63; hind foot, 160; ear (from crown), 85. Skull: total length, 97; basilar length, 76; greatest breadth, 48.2; anterior frontal constriction, 23; length of nasal (longest diagonal), 40; greatest breadth of nasals, 22; alveolar breadth of upper incisors, 9; greatest length of mandible, 76; greatest width of mandible, 47.

The above measurements both of body and skull are a very fair average of the dimensions of five adults taken for Mr. Bangs in Newfoundland by the same collector, Mr. Ernest Doane. Summer specimens from northern Labrador are inseparable from those taken in the same month in Newfoundland. A summer series from Great Slave Lake may show the existence of another race of *arcticus* in that region.

LEPUS GRŒNLANDICUS sp. nov. Greenland Polar Hare.

Tpye, No. 1486 ad. ♂. Col. of Acad. Nat. Sciences, Philadelphia. Collected by Chas. E. Hite at Robinson's Bay, North Greenland, Aug. 2, 1892, for the Peary Relief Expedition.

Description.—Size larger than *L. timidus* L. of Sweden, with radically distinct coloration and incisor dentition.

Adult summer pelage, white, suffused with light tawny and sparingly sprinkled with gray on upper head and ears. Back with scattering black and gray hairs. Tip of ears black. Tail, sides and lower sur-

faces pure white. Half grown young in July and August like adult, but darker, owing to greater abundance of colored hairs and the leaden under fur. Appearances of young and old at a distance at all seasons, white.

Winter pelage, pure white throughout, except the tips of ears, which are black.

Skull with rostral portion anterior to pm. 1, relatively much longer and more attenuate, owing to the outward prolongation of the pre-maxillaries and the small calibre of incisors. Upper and lower incisors very long, produced and slender, their transverse diameter being less than the longitudinal. Upper incisors describe the arc of a circle whose radius is one-fifth ($\frac{20}{100}$) the basilar length of the skull. The chord of their exposed arcs (with cranium, minus mandibles, resting on a plane horizontal surface) forms an angle of 45° to the horizontal plane. Face of upper incisors multistriate, the normal sulcus peculiar to all other members of the genus being so filled with dentine in adult *grœnlandicus* as to obliterate the depression, presenting an even, rounded, enameled contour marked with three minute striæ.

Roots of upper incisors based on the maxillaries and reaching back nearly half way from inferior maxillo-premaxillary sutures to pm. 1. Roots of lower incisors extending to the base of pm. 2.

Measurements (of type taken from dry mounted skin, relaxed): ear, from crown, 100 millimeters; hind foot, 145; tail vertebræ (dry), 50?

Skull: total length, 100; basilar length, 84.5; greatest breadth, 50; anterior frontal constriction, 22.5; length of nasals (longest diagonal), 41; greatest breadth of nasals, 20.5; alveolar breadth of upper incisors, 8.5; greatest length of mandible, 76; greatest width of mandible, 48.

Five skins, seven skulls, and one skeleton, all from North Greenland, comprise the Academy series of Greenland Hares, and all confirm the peculiar characters of this species as above given. I regret that more complete body measurements are not available. Average adult measurements of ear and hind foot are 100 millimeters for the former and 145 for the latter. The total length of an adult skeleton (ligamentous) is 519 millimeters, measured as in the flesh, from tip of nose to end of tail vertebræ.

It is possible that Spitzbergen and Iceland Hares are of the same type as those of Greenland. None of these have come into my hands. The Bavarian, Swiss, Scottish, Irish and Siberian representatives of *timidus* are also likely to prove separable, at least into definable races, already named. From what is known of Linnæus at the time of writ-

ing his tenth edition of the System, it is most fitting that the Polar Hare of Southern Scandinavia should be made the type of the *timidus* group, the Swedish Hares being those which would most naturally embody and form the source of his original diagnosis.

The writer is now preparing a more compendious revision, with illustrations, of the New World representatives of the *Lepus timidus* group, which will probably appear in a future number of the Proceedings of the Academy of Natural Sciences of Philadelphia.

—SAMUEL N. RHOADS.

ENTOMOLOGY.[1]

On Certain Geophilidæ Described by Meinert.—The Chilopoda of the Museum of Comparative Zoology were studied by Dr. Meinert, and the results published in a paper entitled "*Myriapoda Musei Cantabrigensis.*"[2] Many new species were described, but as no figures were given, identification is not in all cases easy, although the descriptions are of considerable length. With reference to the Geophilidæ, at least, there are certain misleading statements and unfortunate omissions. During a recent visit to Cambridge I had the pleasure of a very brief examination of the types of several of Dr. Meinert's species, and some long-standing curiosity was satisfied.

Geophilus georgianus Meinert.

According to Dr. Meinert this species has but a single pleural pore. For some years past I have had specimens from the South which agreed well with the description of this species, but had two pores. As this character is a very constant one, my determination was not made with confidence. The type of *georgianus* has, however, two large pores on each side concealed under the last ventral plate, so that the anomaly is disposed of. The pores are similar in structure and location to those of *G. rubens.*

Geophilus cephalicus Wood.

The specimens described by Meinert, and previously by Wood as *cephalicus* belong to *G. rubens* Say. I have examined the type in the British Museum. It is the most common geophilid in the northeastern states.

Geophilus urbicus Meinert.

No ventral pores could be made out. The sterna are uneven and the whole animal is very hairy. The form of the body, the armature

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

[2] Proc. Am. Phil. Soc. XXI, pp. 161–233 (1885).

of the prehensorial legs and other strong similarities leave little doubt that this is a member of the genus Escaryus, as was conjectured when that genus was erected.[3] The anal legs were also strongly curved under as has been the case with all the specimens of Escaryus yet observed. That the differences enumerated between *E. phyllophilus* and *E. urbicus* can be maintained, is doubtful, for the Cambridge specimen is in rather poor condition, so that some of the characters ascribed by Dr. Meinert may easily prove to have been accidental.

Scolioplanes robustus Meinert.

The locality of this species was not known. I have collected what is evidently the same in central New York and southern Pennsylvania, and am unable to separate it from Sager's *Strigamia fulva*, the probable type of which I have seen in the Museum of the Academy of Natural Sciences at Philadelphia. The only difference between it and *bothriopus* and *robustus* seems to be that of size. The large specimens always show evidences of good living. The creatures are also constructed so as to be capable of considerable distention, besides being variable in size and number of legs, even in the same localities.

Scolioplanes parviceps Meinert.

The label in this bottle, probably in Meinert's handwriting is "*Scolioplanes parviceps* n. sp." The bottle also contains a label marked "*Strigamia bidens* Wood, N. A. loc.?" It is evidently to this label that Dr. Meinert refers when he says, (p. 226). "A specimen, which was said to be a type of Dr. Wood, was labeled '*Strigamia bidens* Wood.'" To thus rename a type specimen seems a remarkable proceeding, especially when the new name proposed has already been used in the same genus. Yet this is probably what Dr. Meinert proposed to do, for Mr. Henshaw kindly showed me a list of the collection, carefully made out in Dr. Meinert's handwriting, and in this the species is again given as new. That it did not so appear when the paper was printed, may have been the work of some American editor who knew of Wood's species and naturally supposed that the same was intended by Meinert.

Wood's *parviceps* is a Californian species, while *bidens* is found in the East. I have collected it in the vicinity of Washington. I had a specimen of *parviceps* at Cambridge with me to compare, but the difference was evident. There was no other specimen of *bidens* at hand, but the size, form of the body and other characters agree well with the eastern species.

[3] Proc. U. S. Nat. Museum XIII, p. 394 (1890).

Scolioplanes longicornis Meinert.

This species was looked upon by its author as the probable type of a new genus. The prehensorial claws are very long and slender, and the basal tooth very small. That it represents a new genus is well-nigh certain, but it would be idle to name it until drawings can be made.

Scolioplanes exul Meinert.

This is a large specimen with a strong general resemblance to large males of *fulvus* (*robustus*). The last pleuræ are without pores except close under the edge of the last ventral plate, where there is a large porose cavity. Anal legs with the claw minute, almost rudimentary, in this offering a strong contrast to the other American species known to me. The anal legs are also very robust, much stouter than a Californian specimen of *parviceps*.

Mecistocephalus breviceps Meinert.

The type specimen is minus the cephalic lamina and antennæ. There is another specimen labeled *breviceps*, but with no locality given. If the type was really collected at Nantucket the species must be very rare or local, for it seems not to have been found elsewhere.

Mecistocephalus heros Meinert.

It has been conjectured by Mr. Pocock that this species should be added to the long list of synonyms of *punctifrons*. I have never examined carefully authentic specimens of *punctifrons*, but the form of the prehensorial legs in the Cambridge specimen, especially the armature of the coxa is different from that of *Haase's* diagram of *punctifrons*. There is no distinct tooth, only a rounded prominence at the distal corner.

Himantarium indicum Meinert.

This specimen is in poor condition and has evidently been allowed to dry at some time in its history. The antennæ are distinctly attenuate. The ventral pores are in a posterior, transverse, subreniform area three or four times as broad as long. This area is scarcely depressed, but is quite definite. Pleural pores are not visible.

Himantarium tæniopse (Wood).

Ventral pores in a small, round, impressed, posterior area. No pleural pores visible, but they may be concealed under the very broad last ventral plate, as is the case in the following species.

Himantarium laticeps (Wood).

The ventral plates appear to be unusually long. The pores are located about two-thirds back, in broad, short, transverse areas. Three

large pleural pores, subconcealed. There seemed to be no specimen of *Himantarium insigne* Meinert in the collection.—O. F. Cook.

Life-history of Scale Insects.—In an excellent account of the Scale Insects affecting deciduous fruit trees Mr. L. O. Howard discusses[4] the life-history of the Coccidæ as follows: In respect to life history, the family Coccidæ, which includes all of the so-called scale insects, is very abnormal. The eggs are laid by the adult female either immediately beneath her own body or at its posterior extremity. Certain species do not lay eggs, but give birth to living young, as do the plant lice. This abnormal habit is not characteristic of any particular group of forms, but is found with individual species in one or more genera. The young on hatching from the eggs are active, six-legged, mite-like creatures which crawl rapidly away from the body of the mother, wander out upon the new and tender growth of the tree, and there settle, pushing their beaks through the outer tissue of the leaf or twig and feeding upon the sap. Even in this early stage the male insect can be distinguished from the female by certain differences in structure. As a general thing, the female casts its skin from three to five times before reaching the adult condition and beginning to lay eggs or give birth to young. With each successive molt the insect increases in size and becomes usually more convex in form. Its legs and antennæ become proportionately reduced, and its eyes become smaller and are finally lost. As a general thing, it is incapable of moving itself from the spot where it has fixed itself after the second molt, although certain species crawl throughout life. The adult female insect, then, is a motionless, degraded, wingless, and, for all practical purposes, legless and eyeless creature. In the armored scales she is absolutely legless and eyeless. The mouth parts, through which she derives nourishment, remain functional, and have enlarged from molt to molt. Her body becomes swollen with eggs or young, and as soon as these are laid or born she dies.

The life of the male differs radically from that of the female. Up to the second molt the life history is practically parallel in both sexes, but after this period the male larva transforms to a pupa, in which the organs of the perfectly developed, fledged insect become apparent. This change may be undergone within a cocoon or under a male scale. The adult male, which emerges from the pupa at about the time when the female becomes full grown, is an active and rather highly organized creature, with two broad, functional wings and long, vibrating antennæ.

[4] Yearbook U. S. Dept. Agr., 1894.

The legs are also long and stout. The hind wings are absent, and are replaced by rather long tubercles, to the end of each of which is articulated a strong bristle, hooked at the tip, the tip fitting into a pocket on the hind border of the wings. The eyes of the male insect are very large and strongly faceted. The mouth parts are entirely absent, their place being taken by supplementary eye spots. The function of the male insect is simply to fertilize the female, and it then dies. The number of generations annually among bark lice differs so widely with different forms that no general statement can be made.

EMBRYOLOGY.[1]

The development of Isopods.—Last Winter when M. Louis Roule published a long paper in French on the development of an Isopod, *Porcellio scaber* Leach, it seemed advisable to present a rather full abstract in this magazine, for the benefit of those readers who would not see the original or who did not read French. That abstract appeared in February and contained, besides the descriptive account of the embryology, some interesting conclusions based on these results.

In the May number of the Journal of Morphology Dr. J. Playfair McMurrich publishes a long paper, illustrated with excellent figures, which is not at all reconcilable with M. Roule's views. It must be remembered, in comparing the two papers, that M. Roule studied a single species of Isopod, that he gives rather diagrammatic figures, and that his description of the segmentation, on which apparently the whole fabric rests, is of a very general nature.

Dr. McMurrich took up the work in 1890, hoping to make out the cytogeny of a Crustacean as Whitman had done for Clepsine, and as E. B. Wilson has later done for Nereis and other forms. This author's results rest then on a thorough study of the segmentation, and as he did not confine his attention to one form, but observed and figured the segmentation and early differentation in a number of Isopods, the paper is of especial interest.

The forms studied were *Jæra marina* Möbius (1873); *Asellus communis* Say; *Porcellio scaber; Armadillidium vulgare;* with some observations on *Cymothoa* and *Ligia*.

The segmentation is centrolecithal. The nucleus of the unsegmented ovum lies in a central mass of protoplasm surrounded by yolk, and

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts, reviews and preliminary notes may be sent.

from the central protoplasm a network extends out through the yolk to a peripheral layer. It is possible to determine the second plane of division as that of the long axis of the embryo as has been shown to be the case in Nereis, Crepidula, and Umbrella. In discussing the segmentation, the author uses the term cell as a convenient one for the nucleated bodies of protoplasm which appear on division in the yolk, but he insists on the syncitial nature of the ovum to a late period.

The cleavage is apparently a spiral one, and results in what may be spoken of as a blastula stage, in which considerable differentiation has taken place. From the eight cell stage, especially in Jæra, it is possible to trace the history of different areas of this blastula to certain well marked cells. For instance, in Jæra, a cell at the posterior pole gives rise to the future Vitellophags, three cells immediately encircling it are the ancestors of the mesendoderm, while the ectoderm arises from the remaining four anterior cells. There are some interesting variations in this history in the forms studied, though the end result is practically the same. The author concludes with E. B. Wilson that, " cells having precisely the same origin in the cleavage, occupying the same position in the embryo, and placed under the same mechanical conditions may nevertheless differ fundamentally in morphological significance."

In connection with the segmentation Dr. McMurrich thinks that the existence of a syncitium up to so late a period in differentiation is of special interest in relation to the current disscusion of the cell-theory. The question is asked, " are we to believe that there is no continuity in Lucifer, between the blastomeres, notwithstanding that in all probability there was continuity in the ova of its ancestors ? " In *Peripatus capensis* there is an approach to holoblastic cleavage associated with less yolk and still a syncitium results. This is regarded as supporting " the supposition that, even in such cases as Lucifer, there may be also a continuity of protoplasm, the separation into distinct spherules being only apparent."

It should be remembered that, however, plausible this argument is, of course the fact of continuity between the blastomeres of Lucifer or of other holoblastic ova still remains to be proven by direct observation.

The conclusion that " the existence of a syncitium is no bar to a certain amount of differentiation," certainly seems justified from the facts described for Jæra. Continuing this subject, such a syncitium is compared with the differentiation in certain protozoa, and a peculiar phenomenon in Porcellio is considered, where there is a precocious segregation of a portion of the cytoplasm which is to take part in the

formation of the blastoderm. This segregation is said to take place, "not in accordance with any previous location of a nucleus, but independently." Dr. McMurrich thinks that "this phenomenon seems to demonstrate that cytoplasmic differentiation may occur *independently of definite nuclear influence.*" He immediately adds, however, that "he, of course does not mean to assert that the nuclei may not possess a *coördinating or even a trophic action upon the cytoplasm,* but that they are directly responsible for the segregation or concentration seems to him an unwarranted assumption."

It is difficult to understand just what is meant by these statements. The remarkable concentration of the peripheral protoplasm of the ovum of Porcellio toward the definitive ventral side, independently of any previous location of nuclei, is notewothy. Does it, however, "demonstrate cytoplasmic *differentiation* independent of definite nulcear influence?" Can this *movement* of protoplasm, even toward a definite point, be correctly spoken of as *differentiation* and compared with the specialization in the cytoplasm of certain protozoa? Having in mind the condition of the ovum when this phenomenon takes place, is it not possible that the movement may be the result of nuclear influences from the center, acting through the central network on the periphal protoplasm?

Again, if the phenomenon demonstrates cytoplasmic differentiation *independent* of definite nuclear influence, why does the author add that, "he does not mean to assert that the nuclei may not possess a *coördinating* action upon the cytoplasm?" There seems to be a contradiction in these two statements, which may destroy the force of the argument. It should also be remembered that in Jæra, and in the other Isopods studied, there is a contraction of the blastoderm cells toward the ventral surface. Here the nuclei, as well as the cytoplasm, of the blastoderm are evidently directly involved. Perhaps the precocious segregation of cytoplasm ventrally in Porcellis is but an early appearance of this process. If it be admitted that the nuclei possess coördinating influences on the cytoplasm, how can it be claimed that in the case of the highly differentiated protozoa such influences were not active during the differentiation?

Another point discussed is the extent of external influences and their action, in holoblastic and in centrolecithal ova like those of Jæra. The conclusion reached from a review of the facts of segmentation is that "the cleavage form of Jæra is determined entirely by intrinsic conditions." The phenomena of segmentation "leave us no choice but to refer the vis essentialis which determines the direction of the Karyokinetic

spindle, and therefore, the cleavage form of Jæra, to the constitutional peculiarity of the ovum." "Holoblastic ova, the author believes, can not be excluded from the action of external forces, but the presumption is allowable, for several reasons, that even in these intrinsic forces are important." The assumption must consequently be made "that intrinsic forces reside in all ova, though they may be overshadowed by external influences in some cases."

It is important to examine the assumption which forms the foundation of this argument. The author quotes E. B. Wilson's conclusion that "cleavage forms are not determined by mechanical conditions alone," and assumes that by "mechanical conditions," Wilson means conditions extrinsic to the ovum. This can hardly be so, for it is necessary to include among the "mechanical conditions" influencing the cleavage of an ovum like that of Jæra, the presence in the cytoplasm of a great accumulation of food-yolk, (excessive in quantity when compared with that in holoblastic or meroblastic ova). It is true that this mass is within the ovum, and in so far "intrinsic", but its action is usually looked on as that of a foreign body, so to speak, which modifies and obscures the primitive phenomona of cleavage and differentiation as seen in holoblastic ova. Hence it is important to remember that Dr. McMurrich, in maintaining that "the cleavage form of Jæra is determined entirely by intrinsic conditions," must include the action of the nutritive mass. This would seem to weaken materially the position that extrinsic influences, (in the generally accepted sense as extrinsic to active cytoplasm), are excluded from action on the spindles of centrolecithal ova. The confusion seems to lie in the use of the word intrinsic to include, in the case of the ovum of Jæra, both inherent properties of the protoplasm, and secondary forces due to the presence of a body of nutritive material which is morphologically not a part of the protoplasm. Dr. McMurrich's conclusion, that "instrinsic forces reside in all ova", or preferably, as E. B. Wilson has just it, "cleavage forms are not determined by mechanical conditions alone," will probably be accepted as truth by most observers. However, I can not see that he has shown that "in Jæra we have practically a demonstration of the correctness of this view" of a more convincing character than is exhibited by holoblastic ova.

"The cleavage form of Jæra, is said to be, determined entirely by intrinsic conditions." A conclusion from which Dr. McMurrich sees no escape, after a review of the changes of position in the yolk assumed by the nuclei during segmentation. The Karyokinetic spindles then are regarded as entirely beyond the influence of forces external to the

ovum in such eggs as those of Jæra, and their direction, with consequently the cleavage form, is due without other alternative entirely to the constitutional peculiarity of the ovum. After carefully considering the evidence presented by Jæra and similar centrolecithal eggs, the assumption does not seem warranted, that they are any more removed from the influences of external forces, than are holoblastic ova. It may be true that it is difficult to *understand* how forces external to a centrolecithal ovum may affect the spindles within it, but many will find the same difficulty in the case of holoblastic ova. Does the great increase of yolk in a centrolecithal ovum remove the spindles from the action of the external world? I, for one, can not see that this necessarily follows, and hence do not see that the condition of segmentation in Jæra leaves us no escape from the conclusion that its cleavage form is determined entirely by intrinsic conditions.

Returning to the description of the embryo, it will be remembered that the germ-layers are already distinguishable in a blastula stage on the surface of the yolk in Jæra, and somewhat less distinctly in the other forms. Now the blastoderm cells gradually concentrate towards the ventral surface of the egg. This results in the mesendoderm and vitellophag cells being crowded beneath the surface in the form of a solid plug, and in the ectoderm of the ventral surface marking out a somewhat triangular area, the base of which lies anteriorly while the apex is posterior. This area is the Nauplius region. The rudiments of the eyes are placed anteriorly at the angles of the base, the appendages appear later along the sides, while the blastoporic plug of mesendodermal cells lies just under the posterior apical end. In a most interesting discussion of the formation of the germs layers in the Crustacea, the author concludes that the primitive Crustacea probably passed through a blastula stage which was filled with yolk, and in which a plug of cells migrated into the yolk to be later differentiated into mesoderm and endoderm. This is the condition exhibited by the Phyllopods (Samassa, 1893 and Bauer, 1892). Jæra, the Decapods and especially Lucifer are examples of precocious differentiation of the germ layers. The entire mesendoderm of Crustacea has a blastoporic origin, and is not (except in Decapods, where there are secondary phenomena) formed by delamination of extra-blastoporic region. The under layer of the latter regions is formed by a migration of cells from the blastoporic plug. In Armadillidium this is especially well made out. An interesting question is raised in regard to the mesenteron of Astacus. Dr. McMurrich suggests the probability that the yolk-pyramids do not form it, but eventually form mesodermic tissues, while

the mesenteron is really formed by cells of the entodermic plates. This interpretation would be more in line with what is known of other Crustacea. Some of the most interesting observations and conclusions of the paper are those concerning the development of the metanaupliar regions of the embryo. It is a remarkable fact that the Naupliar and more posterior metanaupliar regions are very sharply distinguished by differents methods of growth. Dr. Patten was the first to call attention to the fact of teloblastic growth in the ectoderm and mesoderm of Cymothoa. Dr. McMurrich has gone further, and in his comparative study, has made out in detail the character and limits of this method of growth in Isopods. While the Naupliar is formed as described, the metanaupliar regions, are the result of teloblastic growth in ectoderm and mesoderm, just as the metatrochophoral regions of Polygordius are due to a similar process. The author is inclined to regard these two instances of teloblastic growth as acquired independently. He thinks that in the Isopod "the development points back to a period where a free-swimming Nauplius occurred in the development of the ancestors of the group, the egg embryo being a nauplius." At such a time the metanaupliar regions were developed after hatching. Now, however, this posterior region is developed in Isopods before hatching, but it still retains the peculiar teloblastic method of development, and is sharply distinguishable from the Naupliar area.

There is unfortunately not space to describe this remarkable process. It is interesting to note, however, that, while the ectoderm of the metanaupliar regions arises from the successive divisions of a row of ectodermal teloblasts, the rythm of these divisions is not the same as that of the row of mesodermal teloblasts which lies beneath. The mesodermal teloblasts divide just 16 times giving rise to 16 transverse rows of mesoderm cells, "each of which rows is equivalent to a segment," as is proved on the appearance of appendages. The ectodermal teloblasts divide twice as many times.

Though these are the main points of the paper, a number of important observations and conclusions have been necessarily crowded out of this review. For instance, I have not touched on the processes of impregnation, the formation of membranes, the details of segmentation and differentiation, the formation of the digestive tract, the history of the vitellophages, or the development of certain organs.

—H. McE. Knower.

PSYCHOLOGY.

Consciousness and Evolution.—The quotation by Professor Cattell in SCIENCE, July 26, of Professor Cope's table (from the *Monist*, July, 1895) shows that he was equally struck by it with myself. Prof. Cope gives in this table certain positions on points of development, in two contrasted columns, as he conceives them to be held by the two camps of naturalists divided in regard to inheritance into Preformists and the advocates of Epigenesis. The peculiarity of the Epigenesis column is that it includes certain positions regarding consciousness, while the Preformist column has nothing to say about consciousness. Being struck with this I wrote to Professor Cope—the more because the position ascribed to consciousness seemed to be the same, in the main, as that which I myself have recently developed from a psychological point of view in my work on *Mental Development* (Macmillan & Co.). I learn from him that the table[1] is not new; but was published in the the 'annual volume of the Brooklyn Ethical Society in 1891;' and the view which it embodies is given in the chapter on 'Consciousness in Evolution;' in his *Origin of the Fittest* (Appletons, 1887).

Apart from the questions of novelty in Professor Cope's positions—and that Mr. Cattell and I should both have supposed them so can only show that we had before read hastily; I myself never looked into Professor Cope's book until now—I wish to point out that the placing of consciousness, as a factor in the evolution process, exclusively in the Epigenesis column, appears quite unjustified. It is not a question, as Mr. Cattell seems to intimate in his note referred to in SCIENCE, July 26, of a causal interchange between body and mind. I do not suppose that any naturalist would hold to an injection of energy in any form into the natural processes by consciousness; though, of course, Professor Cope himself can say whether such a construction is true in his case. The psychologists are, as Mr. Cattell remarks, about done with a view like that. The question at issue when we ask whether consciousness has had a part in the evolutionary process is, I think, as to whether we say that the presence of consciousness—say in the shape of sensations of pleasure and pain—with its nervous or organic correlative processes, has been an essential factor in evolution; and if so, further,

[1] This table is given in the issue of SCIENCE for July 26, p. 100. The three points from it which are taken up now are cited below.

whether its importance is because it is through the consciousness aspect of it that the organic aspect gets in its work. Or, to take a higher form of consciousness, does the memory of an object as having given pleasure help an organism to get that object a second time? This may be true, although it is only the physical basis of memory in the brain that has a causal relation to the other organic processes of the animal.

Conceiving of the function of consciousness, therefore, as in any case not a *deus ex machina*, the question I wish to raise is whether it can have an essential place in the development process as the Preformists construe that process. Professor Cope believes not. His reasons are to appear fully in his proposed book. I believe that the place of consciousness may be the same—and may be the essential place that Mr. Cope gives it in his left-hand column and which I give it in my *Mental Development*—on the Preformist view. I have argued briefly for this indifference to the particular theory one holds of heredity, in my book (Chap. VII.), reserving for a further occasion certain arguments in detail based upon the theory of the individual's personal relation to his social environment. The main point involved, however, may be briefly indicated now, although, for the details of the social influences appealed to, I must again refer to my book (Chaps. on 'Suggestion' and 'Emotion').

I have there traced out in some detail what other writers also have lately set in evidence, *i. e.*, that in the child's personal development, his ontogenesis, his life history, he makes a very faithful reproduction of his social conditions. He is, from childhood up, excessively receptive to social suggestion; his entire learning is a process of conforming to social patterns. The essential to this, in his heredity, is excessive instability, cerebral balance and equilibrium, a readiness to overflow into the new channels which his social environment dictates. He has to learn everything for himself, and in order to do this he must begin in a state of great plasticity and mobility. Now, my point, but briefly, is that these social lessons which he learns for himself take the place largely of the heredity of particular paternal acquisitions. The father must have been plastic to learn, and this plasticity is, as far as evidence goes, the nervous condition of acute consciousness; the father then learned, through his consciousness, from his social environment. The child does the same. What he inherits is nervous plasticity and the consciousness. He learns particular acts for himself; and what he learns is, in its main line, what his father learned. So he is just as well off, the child of Preformism, as if he had been the heir of the particular lessons of his father's past. I have called this process 'Social Hered-

ity,' since the child really inherits the details; but he inherits them from society by this process of social growth, rather than by direct natural inheritance.

To show this in a sketchy way, I may take the last three points which Professor Cope makes under the Epigenesis column, the points which involve consciousness, and show how I think they may still be true to the Preformist if he avail himself of the resource offered by 'Social Heredity.'

I do this rather for convenience than with any wish to controvert Professor Cope; and it may well be that his later statements may show that even this amount of reference to him is not justified.

1. (5 of Cope's table). "Movements of the organism are caused or directed by sensation and other conscious states."

The point at issue here between the advocate of Epigenesis and the Preformist would be whether it is necessary that the child should inherit any of the particular conscious states, or their special nervous dispositions, which the parent learned in his lifetime, in order to secure through them the performance of the same actions by the child. I should say, no; and for the reason—additional to the usual arguments of the Preformists—that 'Social Heredity' will secure the same result. All we have to have in the child is the high consciousness represented by the tendency to imitate the parent or to absorb social copies, and the general law now recognized by psychologists under the name of Dynamogenesis—*i. e.*, that the thought of a movement tends to discharge motor energy into the channels as near as may be to those necessary for that movement.[2] Given these two elements of endowment in the child, and he can learn anything that his father did, without inheriting any particular acts learned by the parent. And we must in any case give the child this much; for the principle of Dynamogenesis is a fundamental law in all organisms, and the tendency to take in external 'copies' by imitation, etc., is present in all social animals, as a matter of fact.

The only hindrance that I see to the child's learning everything that his life in society requires would be just the thing that the advocates of Epigenesis argue for—the inheritance of acquired characters. For such inheritance would tend so to bind up the child's nervous substance in fixed forms that he would have less or possibly no unstable substance left to learn anything with. So, in fact, it is with the animals in which instinct is largely developed; they have no power to learn any thing new, just because their nervous systems are not in the mobile

[2] Both of these requirements are worked out in detail in my book.

condition represented by high consciousness. They have instinct and little else. Now, I think the Preformist can account for instinct also, but that is beside the point; what I wish to say now is that, if Epigenesis were true, we should all be, to the extent to which both parents do the same acts (as, for example, speech) in the condition of the creatures who do only certain things and do them by instinct. I should like to ask of the Neo-Lamarckian: What is it that is peculiar about the strain of heredity of certain creatures that they should be so remarkably endowed with instincts? Must he not say in some form that the nervous substance of these creatures has been 'set' in the creatures' ancestors? But the question of instinct is touched upon under the next point.

2. (6 of Cope's table). "Habitual movements are derived from conscious experience." This may mean movements habitual to the individual or to the species in question. If it refers to the individual it may be true on either doctrine, provided we once get the child started on the movement—the point discussed under the preceding head. If, on the other hand, habitual movements mean race movements, we raise the question of race habits, best typified in instinct. I agree with Mr. Cope that most race habits are due to conscious function in the first place; and making that our supposition, again we ask: Can one who believes it still be a Preformist? I should again say that he could. The problem set to the Preformist would not in this case differ from that which he has to solve in accounting for development generally: it would not be altered by the postulate that consciousness is present in the individual. He can say that consciousness is a variation, and what the individual does by it is 'preformed' in this variation. And then what later generations do through their consciousness is all preformed in the variations which they constitute on the earlier variations. In other words, I do not see that the case is made any harder for the Preformist by our postulate that consciousness with its nervous correlate is a real agent. And I think we may go further and say that the case is easier for him when we take into account the phenomena of Social Heredity. In children, for example, there are variations in their mobility, plasticity, etc.; in short, in the ease of operation of Social Heredity as seen in the acquisition of particular functions. Children are notoriously different in their aptitudes for acquiring speech, for example; some learn faster, better, and more. Let us say that this is true in animal communities generally; then these most plastic individuals will be preserved to do the advantageous things for which their variations show them to be the most fit. And the next generation will

show an emphasis of just this direction in its variations. So the fact of Social Heredity—the fact of acute use of consciousness in ontogeny—becomes an element in phylogeny, also, even on the Preformist theory.

Besides, when we remember that the permanence of a habit learned by one individual is largely conditioned by the learning of the same habits of others (notably of the opposite sex) in the same environment, we see that an enormous premium must have been put on variations of a social kind—those which brought different individuals into some kind of joint action or coöperation. Wherever this appeared, not only would habits be maintained, but new variations, having all the force of double hereditary tendency, might also be expected. But consciousness is, of course, the prime variation through which coöperation is secured. All of which means, if I am right, that the rise of consciousness is of direct help to the Preformist in accounting for race habits—notably those known as gregarious, coöperative, social.

3. (7 of Cope's table). "The rational mind is developed by experience, through memory and classification." This, too, I accept, provided the term 'classification' has a meaning that psychologists agree to. So the question is again: Can the higher mental functions be evolved from the lower without calling in Epigenesis? I think so. Here it seems to me that the fact of Social Heredity is the main and controlling consideration. It is notorious how meagre the evidence is that a son inherits or has the peculiar mental traits of parents beyond those traits contained in the parents' own heredity. Galton has shown how rare a thing it is for artistic, literary or other marked talent to descend to the second generation. Instead, we find such exhibitions showing themselves in many individuals at about the same time, in the same communities, and under the same social conditions, etc. Groups of artists, musicians, literary men, appear, as it were, as social outbursts. The presuppositions of genius—dark as the subject is—seem to be great power ef learning or absorbing, marked gifts or proclivities of a personal kind which are not directly inherited but fall under the head of sports or variations, and then a social environment of high level in the direction of these sports. The details of the individual development, inside of the general proclivity which he has, are determined by his social environment, not by his natural heredity. And I think the phylogenetic origin of the higher mental functions, thought, self-consciousness, etc., must have been similar. I have devoted space to a detailed account of the social factors involved in the evolution of these higher faculties in my book.

I fail to see any great amount of truth in the claims of Mr. Spencer that intellectual progress in the race requires the Epigenesis view. The level of culture in a community seems to be about as fixed a thing as moral qualities are capable of being; much more so than the level of individual endowment. This latter seems to be capricious or variable, while the former moves by a regular movement and with a massive front. It would seem, therefore, that intellectual and moral progress is gradual improvement, through improved relationships on the part of the individuals to one another; a matter of social accommodation, rather than of natural inheritance alone, on the part of individuals. It is only a rare individual whose heredity enables him to break through the lines of social tissue and imprint his personality upon the social movement. And in that case the only explanation of him is that he is a variation, not that he inherited his intellectual or moral power Furthermore, I think the actual growth of the individual in intellectual stature and moral attainment can be traced in the main to certain of the elements of his social *milieu*, allowing always a balance of variation in the direction in which he finally excels.

So strong does the case seem for the Social Heredity view in this matter of intellectual and moral progress that I may suggest an hypothesis which may not stand in court, but which I find interesting. May not the rise of the social life be justified from the point of view of a second utility in addition to that of its utility in the struggle for existence as ordinarily understood; the second utility, *i. e.*, of giving to each generation the attainments of the past which natural inheritance is inadequate to transmit? Whether we admit Epigenesis or confine ourselves to Preformism, I suppose we have to accept Mr. Galton's law of Regression and Weismann's principle of Panmixia in some shape. Now when social life begins we find the beginning of the artificial selection of the unfit; and so these negative principles begin to work directly in the teeth of progress, as many writers on social themes have recently made clear. This being the case, some other resource is necessary besides natural inheritance. On my hypothesis it is found in the common or social standards of attainment which the individual is fitted to grow up to and to which he is compelled to submit. This secures progress tn two ways: First, by making the individual learn what the race has learned, thus preventing social retrogression, in any case; and seecond, by putting a direct premium on variations which are socially available.

Under this general conception we may bring the biological phenomena of infancy, with all their evolutionary significance: the great

plasticity of the mammal infant as opposed to the highly developed instinctive equipment of other young; the maternal care, instruction and example during the period of helplessness, and the very gradual attainment of the activities of self-maintenance in conditions in which social activities are absolutely essential. All this stock of the development theory is available to confirm this view.

And to finish where we began, all this is through that wonderful engine of development, consciousness. For consciousness is the avenue of all social influences.—J. MARK BALDWIN, Princeton.

The preceding communication from Prof. Baldwin is copied from Science of August 23, 1895. It is reprinted in order to render intelligible a review of it which I propose to publish in the next number of the NATURALIST.—E. D. COPE.

ANTHROPOLOGY.[1]

Mercer's Cave Explorations in Yucatan.[2]—This a handsomely illustrated volume which describes in detail the researches made by the Corwith Expedition to Yucatan, under the direction of Mr. H. C. Mercer of the University of Pennsylvania. The object of the expedition was to search for the remains of prehistoric man in the cave deposits, and to learn who were the predecessors or ancestors of the peoples whose civilization is attested by the remarkable ruins which are such a conspicuous feature of that country. Explorations of this kind made in Europe have achieved such important results to archeology, that every research in America must be watched with great interest. As a summary of his work, Mr. Mercer remarks:

"The intervening two months seemed a long time; nor was it easy to realize that, after all, the area gone over had not exceeded one hundred miles in length by ten in breadth. Twenty-nine caves had been visited in sixty days, of which ten had been excavated. Thirteen had archeological significance. Six had yielded valuable, and three, decisive results.

"We had seen but little of the ruins. We had not passed southward over the boundary line into the great wilderness, whence fables of lost cities reach the traveller's ear. Our continued study of an un-

[1] This department is edited by H. C. Mercer, University of Pennsylvania.

[2] The Hill Caves of Yucatan: A Search for the Evidence of Man's Antiquity in Central America; being an account of the Corwith Expedition of the Department of Archeology and Paleontology of the University of Pennsylvania, by Henry C. Mercer. J. B. Lippincott & Co. Philadelphia, 1896. 8vo., pp. 183.

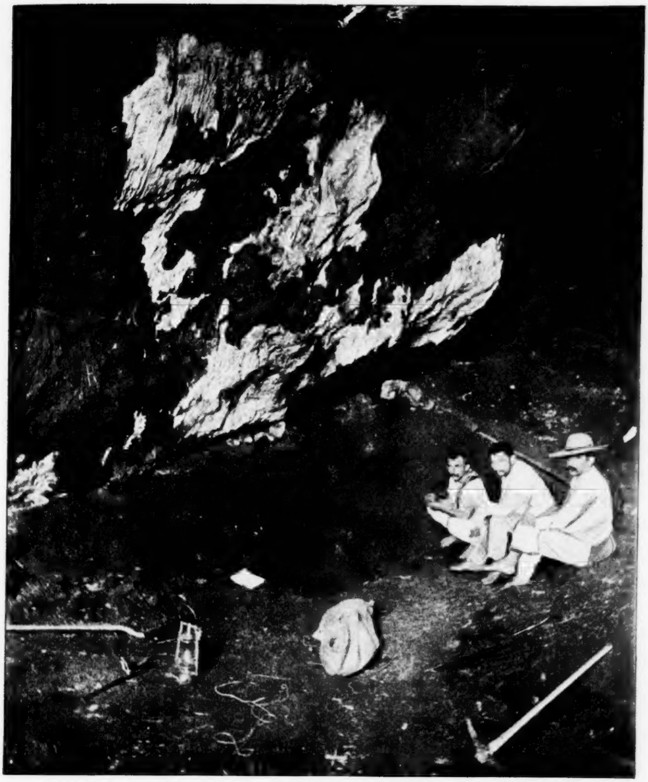
derground layer of human refuse substantially the same in all the caves, instructive as it was, had taught us but little of details. Evidently a wide range of tools and implements had not been left, lost or broken in the subterranean rooms. We did not find, and did not expect to find, that the water producing underground chambers had been used as burying places. Neither were they dwellings, but rather temporary halting spots, which, but for the water supply, would probably have shown fewer human traces than do the caves of the United States. Human bones scattered in the rubbish indicated that the old inhabitants of Yucatan practiced cannibalism. Beyond that, the traces of pre-Columbian cookery at the underground sites referred to an ancient cave visitor, who was rather an agriculturist than a hunter, and who (unless the dog found at Sabaka be an exception) possessed no domestic animals.

"We had learned little of stone chipping, and had found in the scanty list of stone blades but one imperfect point that might have served for an arrowhead. The secret of stone carving we had failed to discover, and though the whole mystery had seemed within our grasp at Oxkintok, we had to rest content with proving that the chiselling of the ruins could not have been done with chips of the parent block or round hammer stones. We had found no copper, or gold, or silver, no jade, no gums, no preserved grains, no cloth, no apparatus for weaving, and had discovered no pipe, and learned nothing of pre-Columbian smoking or tobacco.

"A close examination of the potsherds showed a ware mixed with powdered limestone that reacted strongly under acid on the fractures. A smooth red make, strong, wellbaked, and symmetrical, and whose dull polished surface resisted the action of nitric acid, was abundant, while a very few fragments were decorated with brightly colored designs, though their polish, after the manner of varnish, yielded readily to the acid test. Many, though better baked than the ware of the Delaware Indians, were coarse. A very common hard variety had been striped with brown lines on a white or bluish background. But there was nothing brilliant or striking about these fragments of dishes, cooking pots, or water jars. Few were ornamented, and only two or three highly so. None were marked with hieroglyphs. Nevertheless, a variety of tones, colors, and polish struck the eye when many sherds were laid side by side and brushed.

"But results more important than these had rewarded our close examination of the position and contents of the human rubbish heap everywhere present in the caves. Though this layer was the only cul-

PLATE VI.



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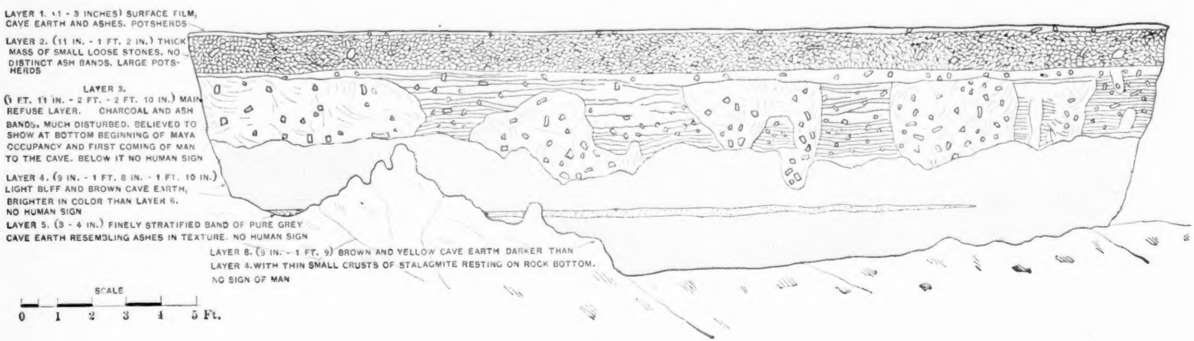
1. Cave of Sayab Actun, interior. 2. Section of Cave of Actun Xmak.

PLATE VII.



Section of Cave of Loltun.

ture layer, our digging had fairly proved at Oxkintok, Loltun and Sabaka, and though we had often failed to reach rock bottom at other caverns, there was nowhere ground for supposing that deeper digging or blasting would have upset our inference. An earlier people visiting Yucatan under its present topographical conditions must needs have left their trace in the caves, and because the undisturbed earth beneath the culture layer discovered, always failed to show trace of any deeper, older or more primitive human visitor, the conclusion was that no such earlier people had seen the region while its stony hills, its torrid plain, and its damp caves were as they now are."

The evidence secured by Mr. Mercer justifies this conclusion so far as it goes. To prove that a human population existed in Yucatan prior to that whose remains were actually found, it will be necessary to discover another series of deposits inside or out of an older type of caves. No such caves were found, and while it cannot be asserted that such will not be found, it is evident that they must be very rare if existing in the region explored. The case of Yucatan may prove to be similar to that of the United States, where I have shown on paleontologic grounds,[3] that cave deposits of two different ages exist. The remains of vertebrate life found in the caves of Yucatan explored by Mr. Mercer, are those of the existing fauna of the country, and the deposits correspond, therefore, with those of the second (postchamplain) age of the northern caves. Caves of prechamplain age are rare in the United States, as shown by Mr. Mercer's earlier researches, having been probably removed by the action of water during the Champlain submergence. That such a submergence may have also taken place in Yucatan is indicated by the recent researches of Spencer; but if so, a cleaner sweep of them was made than was the case in North America.

Among the remains of animals which were discovered, those of the horse occurred in two caves, and the dog in one. It is probable they both belong to the domesticated species.

I append some examples of the very admirable illustrations with which the book abounds.

Apart from its scientific value, this book will interest the general reader for various reasons. It is written in a pleasant style, and many side lights are thrown on the characters of the country and people. That the exploration was not without the element of danger is shown by the tragic death of one of the natives; while the sufferings of the

[3] American Naturalist, 1895, p. 598.

party from heat and insects show that none but hardy explorers could undertake such labor. We recommend the book as an admirable ex-

Interior of grand rotunda of Cave of Actun Benado.

ample of the combination of utility with adventure which characterizes scientific research in the wilds.—E. D. Cope.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

Academy of Science of St. Louis.—President Gray in the chair and twenty-two other persons present, Mr. Trelease exhibited several specimens, about three feet square, of a curious silk tapestry, taken from the ceiling of a corn-storing loft in San Luis Potosi, Mexico, by Dr. Francis Eschauzier, stating that he was informed that the larger specimen had been cut from a continuous sheet over twenty yards wide and about four times as long. The specimens, of a nearly white color, and of much the appearance and feeling of a soft tanned piece of sheepskin, were shown to be composed of myriads of fine silken threads, crossing and recrossing at every conceivable angle, and so producing a seemingly homogeneous texture. Although specimens of the creatures by which they are produced had not been secured, it was stated that there

was no doubt that these tapestries are the work of lepidopterous larvæ which feed upon grain, the presumption being that they are made by the larvæ of what has been called the Mediterranean Grain or Flour Moth (*Ephestia kühniella*). The speaker briefly reviewed the history of this insect and its injuriousness in various parts of the world, and quoted from a report of Dr. Bryce, showing that in Canada, where it became established in 1889, "a large warehouse, some 25 feet wide, 75 feet long, and four stories high, became literally alive with moths in the short course of six months."—WILLIAM TRELEASE.

Boston Society of Natural History.—February 5th.—The following paper was read: Mr. Herbert Lyon Jones, "Biological adaptations of desert plants to their surroundings.—SAMUEL HENSHAW, *Secretary.*

Nova Scotian Institute of Science.—13th of January.—The following papers were read: "Notes on the Superficial Geology of Kings County, Nova Scotia," by Prof. A. E. Coldwell, M. A., Acadia College. "A Note on Newton's Third Law of Motion," by Prof. Mac Gregor, D. Sc., F. R. SS. E. & C., Dalhousie College.—HARRY PIER, *Secretary.*

New York Academy of Science, Biological Section.—January 13th, 1896.—The papers presented were: G. S. Huntington on "*The Visceral Anatomy of the Edentates.*" The characters of the brain, alimentary, respiratory and genito urinary tracts were especially considered. The following forms were discussed: *Myrmecophaga jubata, Tamandua bivittata, Arctopithecus didactylus, Dasypus sexcinctus, Tatusia novemcincta, Manis longicaudata.* In the brain characters the following features were considered;—the transverse frontal sulcus, the great longitudinal fissure, and the absence of a distinct Sylvian fissure. In the alimentary tract the Sloths are to be sharply separated from the remaining groups, the stomach structure with its pyloric gizzard notably aberrant: the ileo-colic junction is traced throughout the edentates in a well marked series of transitional forms.

O. S. Strong, "*On the Use of Formalin in Injecting Media.*" The paper made especial note of the advantages possessed by this preservative in injecting the brain *in situ.* Formalin (40 per cent formaldehyde) diluted with an equal volume of water is injected into the cephalic vessels until it runs from the cut jugulars. After a few minutes the same quantity is again injected and once or twice again after an elapse of fifteen to twenty minutes. The brain is then removed and will be found to be completely fixed throughout. The swelling usually

noticed in formalin hardened brains does not appear to take place when this method is employed. Besides the many general advantage of fixing brains by injection, formalin has the especially merit of giving them the best consistency for macroscopic work, and further such brains are available subsequently for the Golgi and Weigert methods as well as, possibly, for cytological methods. Formalin has also the advantage that it can be used, as above, stronger than is necessary for fixation and thus allowance made for its dilution when permeating the tissue. When only the Golgi method is to be used, an equal volume of a 10 per cent solution of potassium bichromate may be added to the formalin instead of water. Pieces can be subsequently removed, hardened further in formalin-bichromate and impregnated with silver.

Bashford Dean, "*On the Supposed Kinship of the Paleospondylus.*" A favorably preserved specimen of this interesting fossil, received by the writer from Wm. T. Kinnear of Forss, Scotland, appears to warrant the belief that this lamprey-like form was possessed fins, a character decidedly adverse to the now widely accepted view of Marsipobranchian affinities. The structure referred to consists of a series of transversely directed rays, arising from the region of the postoccipital plates of Traquair. From this peculiar character, as well as from many unlamprey-like features of the fossil, it would appear accordingly that the kinship of the Paleospondylus is as yet by no means definitely determined.—C. L. BRISTOL, *Secretary.*

Nebraska Academy of Sciences.—The following program of papers was presented. *First Session—Thursday, Jan.* 2, 1896. "America the Primitive Home of Civilization," H. S. Clason ; "The Home of the Buffalo Grass," Dr. C. E. Bessey; "Early Rainfall Records in Nebraska," G. D. Swezey; "The Volcanic Ashes of Nebraska," Dr. E. H. Barbour. *Second Session—Friday, Jan.* 3.—"The Relative Importance of Economic Fungi, East and West," F. W. Card; "Animal Parasites of Nebraska," Dr. H. B. Ward; "Diatomaceous Deposits of Nebraska," Dr. E. H. Barbour; "Some Fossil Diatoms from Nebraska," C. J. Elmore; "Wind Velocities in Nebraska," G. A. Loveland; "Report of Progress on the Study of Dæmonelix," Dr. E. H. Barbour; "Origin of the Present Flora of Nebraska," Dr. C. E. Bessey.

SCIENTIFIC NEWS.

Huxley Memorial.—Since the first meeting of the General Committee on November 27, which was fully reported by the Press, two meetings of the Executive Committee have been held.

At the first of these, at which Lord Shand accepted the office of Chairman, it was reported that a number of foreigners of eminence had expressed a wish to be associated with the proposal to commemorate Mr. Huxley's distinguished services to humanity. It was resolved, in the first instance, to invite subscriptions from the members of the General Committee.

At the second meeting, held on December 18, it was reported that the subscriptions, which at the General Meeting had amounted to £557, had been increased to about £1,400, and it was resolved that a wider appeal for subscriptions should now be made to the friends and admirers of Mr. Huxley amongst the general public. The sum subscribed now exceeds £1,500.

The Honorary Secretary stated that in America Committees were in the course of being formed to promote the realization of an adequate fund.

The Committee resolved to communicate, by means of a sub-committee of their number, with Mr. Onslow Ford, R. A., who had the advantage of being well acquainted with Mr. Huxley, in reference to the statue, which it is proposed should be erected beside those of Darwin and Owen in the Natural History Museum, South Kensington.

The extent to which the Committee may be able to carry out the other intended objects of founding exhibitions, scholarships, and medals for biological research and lectureships, and possibly in assisting the republication of Mr. Huxley's scientific works, will, of course, depend on the subscriptions which may now be received.

Meehans' Monthly is a magazine for the lovers of gardening; and covers the whole field of general intelligence in so far as it may have the remotest bearing on the chief topics it sets out to advance. For instance, a beautiful Prang colored plate of some wild flower is given every month, with a description which illustrates the whole ground of classical history that has any bearing on the topic. Information on the most diversified topics abound. Corn from Indian mounds will not grow—swamps that are real swamps are among the healthiest of localities. There is no sickness in the great dismal swamp of Virginia.

Elderberry root is found to be a deadly poison. Foul water is pronounced to be a self-purifier, because bacteria eat out vegetable matter and then die of starvation. The hickory and the chestnut are proven cousin—Germans. Weeds are useful, by forcing the cultivator to work to aerate the soil. Illustrations of a curious maze, formed of yew hedges at Hampton Court, pruning and keeping trees from insects, chrysanthemum culture, and practical information on fruits and flowers are among the topics treated. Sample copies may be had of the publishers, Thomas Meehan & Sons, Germantown, Philadelphia.

In the January *Monist*, of importance to students of evolution will be the article on *Germinal Selection*, by the famous German biologist, Prof. August Weismann, of Freiburg. In the theory of germinal selection, Prof. Weismann propounds a doctrine which rounds off and perfects, as he claims, the theories of Darwin and Wallace, and which consists essentially in applying the principle of the struggle for life to the minutest parts of organization, viz., to the germinal and determinant particles generally. Weismann's article is a complete summary of the present status of the discussions in evolutionary theory, and will itself doubtless constitute one of the most important recent acquisitions to biological science.

Abnormal pleasures and pains are treated by Prof. Th. Ribot, who applies to their explanation the pathological method, using diseases as a means of analysis. His results as regards the pleasure which some people take in pain are highly interesting.

The fourth annual meeting of University Extension and other students will be held in the four weeks beginning July 6, 1896, in the buildings of the University of Pennsylvania. The Summer Meeting combines the advantages of an ordinary summer school with the cooperative feature which distinguishes conventions, or associations, in which there are representatives of many universities and colleges.

Professor E. Selenka, of Erlangen, has resigned his position in order that he may make a scientific journey. He has been appointed Honorary Professor of Zoology in Munich. His place at Erlangen is temporarily filled by Dr. Albert Fleischmann.

The Paris Academy of Science has recently elected the following corresponding members: Dr. G. Retzius, of Stockholm, as successor to Carl Vogt; and Prof. R. Bergh, of Copenhagen, as successor to Huxley.

Dr. F. Miescher, Professor of Physiology in the University of Basel, died at Davos, Switzerland, Aug. 26, 1895, aged 51 years. Dr. Rudolf Metzner, of Freiburg i B, has been appointed his successor.

Dr. Felix Hoppe-Seyler, Professor of Physiological Chemistry in the University of Strassburg, died in Wassenburg, on the Lake of Constance, Aug. 11, 1895, aged 70 years.

The Australian Association for the Advancement of Science, will hold its annual meeting at Sydney, Jan. 3 to 10, 1896. Professor A. Liversedge is the President.

The Berlin Academy of Science has elected Professors K. W. von-Gümbel, K. A. von Zittel, A. Cossa, and Mr. Alexander Agassiz as corresponding members.

Dr. R. Krause, who formerly had charge of the Anthropological Section of the Museum Godfroy in Hamburg, died in Schwerin, Mecklinburg, July 25, 1895.

Dr. A. Schaper, of Zürich, has been appointed instructor in Histology and Embryology in the Harvard Medical School.

Ernst Baumann, formerly head of the station of Misahöhe, West Africa, died in Cologne, Sept. 4, 1895, aged 24 years.

Professor Hellriegel, botanist and Director of the Agricultural Experiment Station in Bernburg, died Sept. 24, 1895.

F. Nies, Professor of Mineralogy and Geology in the Agricultural School of Hohenheim, is dead at the age of 56.

Dr. Herman Credner has been advanced to the Ordinary Professorship of Geology in the University of Leipzig.

Dr. V. Rohon has been appointed Extraordinary Professor of Histology in the Bohemian University in Prag.

Dr. Valentin Häcker has been advanced to Extraordinary Professor of Zoology in the University of Freiburg.

Dr. Emil Yung is the successor of the late Carl Vogt as Professor of Zoology in the University of Geneva.

Dr. Moritz Willkomm, formerly Professor of Botany in Prag, died Aug. 26, in Wortenburg, Bohemia.

Dr. Kallies of Göttingen, has been promoted to Extraordinary Professor of Anatomy in Tübingen.

Joseph Thompson, African explorer and geologist, died in London, Aug. 2, 1895, aged 37 years.

Dr. F. Reinitzer, of Prag, has been appointed Extraordinary Professor of Botany in Graz.

Mr. R. Trimen has resigned his position as Director of the Cape Town (Africa) Museum.

L. Perry Arcas, entomologist, died in Requena, Spain, Sept. 24 1895, aged 70 years.

Dr. D. Brandza, Professor of Botany in Bucharest, died August 15, 1895, aged 48 years.

Dr. A. S. Dogiel, of Tomsk, goes to St. Petersburg as Professor of Histology.

Dr. H. Lenk, of Leipzig, has been called to the chair of Geology in Erlangen.

F. Kitton, the student of diatoms, died at Norwich, England, July 22, 1895.

E. J. Chapman, Professor of Geology in Toronto, has resigned his position.

Dr. F. Czapek is now Privat-docent in Botany in the University of Vienna.

Professor Sven Loven, of Stockholm, died Sept. 4, 1895, aged 86 years.

Dr. H. Strahl has been appointed Professor of Anatomy in Giessen.

Dr. P. H. Macgillivray, the student of Australian Polyzoa, is dead.

Dr. M. Miyoshi has been called to the chair of Botany in Tokyo.

Dr. A. Senoner, geologist, died in Vienna, Aug. 30, 1895.

Dr. J. Vesque, botanist, of Vincennes, France, is dead.

Dr. F. Müller, herpetologist, died at Basel in May.